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HABIT-INTERFERENCE AS DEPENDENT UPON STAGE OF TRAINING

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When two incompatible habits are learned successively so that interference appears in the learning of the second habit, it seems only natural that the stage of training reached in the first habit before shifting to the second should be an important condition influencing the amount of interference. Various researches have shown that the amount of interference is somehow related to the amount of previous training, but as to the direction and the exact nature of the relationship the evidence is inconclusive. Some investigators have found that more training leads to greater interference, while others have found, in direct contradiction, that the amount of interference bears an inverse relation to the amount of previous training. The aim of this research was that of determining directly the nature of the relationship between amount of interference and stage of previous training. A feature of the method was the use of measured proficiency rather than mere frequency of repetition as the criterion for the stages of training.

HISTORY OF THE PROBLEM

This review of the history is limited to the material which deals directly with the specific problem under consideration. Within the tremendous literature on the general topic of interference the point of a relationship between amount of interference and stage of previous training is made more often the subject for casual reference or easy assumption than for experimental treatment. Such evidence as is available comes in many cases from incidental data obtained in the investigation of some other aspect of the general topic.

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Münsterberg was one of the earliest experimenters to touch upon the problem of this paper.[1] He investigated interference by altering automatic daily habits such as the placing of the watch in a certain pocket. He found that with continued practice in the new habit the interference gradually disappeared as the new habit became automatic and replaced the old. With repeated shifting from one habit to the other the mutual interference diminished with each new shift and gradually disappeared entirely. Münsterberg's experiments gave no direct evidence upon the problem of the manner in which different amounts of separate practice in the first habit affect the amount of interference; it is strange that Münsterberg's work has been rather commonly cited upon this specific point.

Bair attempted to find out "whether a habit which has been made permanent with many practices offers more resistance to displacement by a different habit than one formed by only a few practices."[2] Alternative forms of a simple place-habit (*e.g.* card-sorting) were used as the conflicting tasks. Each *S* had first a single trial of the first task, then a single trial of the second task, then 10 repetitions of the first, and finally one more trial of the second. Comparison was made between the amount of interference (increased time) at the first shift following a single trial and the amount of interference at the second shift following 10 trials of the first task. The results showed consistently less interference for the shift after 10 repetitions. In further experiments Bair extended his series to include practice periods of 1, 10, 20, 30, and 40 repetitions between the successive shifts to the second task. The results showed that the greater the number of repetitions of the first task preceding the shift, the smaller was the amount of interference-effect. Bair concluded that interference decreases proportionally to increase in practice of the first task, and that long continued practice may have an actual facilitating effect upon the performance of the second, antagonistic task (p. 45). In drawing these conclusions Bair failed to mention a fact which may have been responsible in large part for the nature of the results obtained. The arrangement of the series in the experiments provided practice in shifting from task to task along with practice in the first task; each *S* went through the entire series. That practice in shifting from task to task is effective in decreasing interference had been shown by Münsterberg and by Bair himself in an earlier experiment. Bair's results, then, justify no simple conclusion as to the nature of the relationship between interference and practice in a first task.

Culler obtained some data upon our problem by the use of choice-reactions.[3] For the first task the *S* reacted to a red light with the right hand and to a blue light with the left; for the second task the hand-color relationships were reversed. One group of *S*s had 140 reactions for each hand before shifting to the second task while the other group had only 60. Interference was measured in terms of the differences in reaction-time between the last 30 trials of the first task and the first 30 trials of the second. The results showed definitely more interference for the group with the greater amount of training in the first task. Culler's conclusions are in direct contradiction to those of Bair.

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[1] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, 1889, IV, Pt. I.

[2] J. H. Bair, The practice curve: A study in the formation of habits, *Psychol. Monog.*, 5, 1902, (no. 19), 35.

[3] A. J. Culler, Interference and adaptability, *Arch. Psychol.*, 3, 1912, (no. 24), 1-80.

Culler's conclusions should be examined carefully with consideration for the manner in which the amounts of interference were determined. In customary usage interference is said to occur when practice in one task makes the performance of a second, incompatible task more difficult or less proficient than it would have been without the previous practice. With speed of reaction as the measure of proficiency, the customary manner of determining amounts of interference would be that of comparing speed at a given stage in the learning of the second task with speed at the same stage in original learning of that task by a control group or, if the two tasks are of the same inherent difficulty, with speed at the same stage in the learning of the first task. Culler, however, compared initial speeds in the second task with final speeds in the first task. His two groups with different amounts of practice arrived at widely different levels of speed in the first task before they were shifted. After the shift the speed for the long-trained group fell far below the high level finally reached by this group in the first task, while the speed for the less-trained group showed less decline from the relatively low level at which it finished the first task. On the basis of this comparison the long-trained group was said to show more interference. This manner of treatment implies a conception of interference quite different from the usual one, and logically it involves the assumption that there is to be expected always, unless interference occurs, an amount of positive transfer in exact proportion to the amount of improvement during practice of the first task. Such an assumption seems wholly unwarranted, and its introduction leads to obvious difficulties. Suppose that the two groups in an experiment similar to Culler's should each start the second task at an initial speed exactly the same as the initial speed for the first task; according to customary notions this case would show an obvious lack of interference for both groups since in each case the new task was performed just as if there had been no previous practice. Subjected to Culler's treatment this case would show that the long-trained group had much more interference. Culler's conclusions, then, must be taken to be strictly dependent upon his method of treating the results, and they can not be regarded as significant for the problem taken in its customary sense.

Kline used paired associates to investigate the relationship between the strength of an original associative bond and the difficulty with which a new association could be substituted.[4] For the original associations such familiar combinations as states and their capitals were used, and the strength of the bond was measured by the number of individuals in a group able to recall the second member of the pair. Interference in the learning of the new association was measured by the increase in reaction-time for the 'false' associates as compared with the times for the original responses and for those of a control series. Kline concluded that interference is maximal when the original bond is of medium strength, that there is less interference when the bond is very strong, and that there is little or no interference when the bond is weak. Kline's results are difficult to compare with those of other researches since his material is so different and especially since strength of the original connection is treated statistically as group frequency.

Wiltbank gave white rats various amounts of training in one maze, then shifted them to a second maze and compared the readiness with which the differently trained

[4] L. W. Kline, An experimental study of associative inhibition, *J. Exper. Psychol.*, 4, 1921, 270-299.

groups learned the new task.[5] Errors, time, and number of trials were used as measures, and percentages of interference or transfer were computed in comparison with the figures for the control group. In terms of errors and number of trials required, interference is definitely shown for the less-trained groups (2 to 8 trials in the first maze), while positive transfer appears for the groups with much practice (16 or more trials). In total time required for learning the second maze, all groups showed a high transfer-effect (75% to 95%) with only slight differences between the groups. In general, Wiltbank's results indicate an inverse relation between interference and training in the first task (in agreement with Bair and to some extent with Kline), although it is difficult to explain the consistent transfer-effect shown by the figures for total time.

Hunter trained rats in light discrimination and then reversed the direction in which they were required to run.[6] One group was given 100 repetitions of the first habit before the shift, and another group was trained until the responses were 95% correct (an average of 286 trials). The members of the highly-trained group required on the average 603 trials to learn the reversed task, while the less-trained group learned the second task in an average of 475 trials. Hunter concluded from his results that when the first task is approximately half learned, the amount of interference is about one half of the total possible amount which results if the first task is completely learned. The results hardly justify the drawing of such a precise relationship, since 'half-training' was not determined by proficiency but was merely implied by the fact that the less-trained group had a uniform number of trials below one half of the average number required for complete learning. However, Hunter's results do indicate that a higher degree of training in the first task results in a greater amount of interference (in agreement with Culler).

The investigations reviewed above seem to include all of the direct evidence available upon our specific problem. There is a striking lack of agreement in the conclusions reached, and the results are not highly comparable. Although a great variety of materials and procedures makes comparison difficult, a more serious difficulty in comparison arises from the fact that stages of training were generally determined by arbitrarily chosen numbers of repetitions and from the lack of uniformity of terms and of places in the curves used for measuring interference. These investigations serve chiefly in offering suggestions for improvement in methods of isolating and attacking the problem.

Some theoretical considerations seem sufficiently important for the problem to be mentioned here. The significance of much of the former work on problems of learning has recently been questioned by psychologists of the *Gestalt* school. In the light of this new criticism the problems have been formerly approached from the standpoint of untenable assumptions which may be termed 'associationistic' in that specific linkages or bonds between elements have been assumed to determine reproduction.[7] In connection with the topic of interference, the associationistic approach is reflected in the law of associative interference (stated in 1894 by Müller and Schumann)

[5] R. T. Wiltbank, Transfer of training in white rats upon various series of mazes, *Behav. Monog.*, 4, 1919, (no. 17), 1-65.

[6] W. S. Hunter, Habit interference in the white rat and in human subjects, *J. Comp. Psychol.*, 2, 1922, 29-59.

[7] W. Köhler, *Gestalt Psychology*, 1929, 269-347.

which puts the matter into terms of a tendency for an established bond between two elements to inhibit the formation of a similar bond between either and a third element. The *Gestalt* approach questions the existence of an active associative tendency which acts as the primary force (*Motor*) to determine reproduction and which must interfere with any attempt to alter the reproductive sequence.

While it is true that early associationists, especially those in Germany, regarded association as an active tendency, such a view is extremely uncommon among later psychologists who might be called the associationistic group. In fact, a more pertinent criticism might concern itself with the vagueness of the present concepts of association and with the fact that the associative tendency has become so static as to be regarded, in extreme views, as a tendency only in the statistical sense of probability. At any rate, we may derive from this *Gestalt* protest the warning that interference is not to be thought of as the inevitable consequence of a persistent force set up by previous association and operating on every occasion to resist change in the original sequence of events.

A second point of protest, one much more directly related to the problem under consideration, is the suggestion that mere frequency of repetition is not important among the conditions determining reproduction and recall. The emphasis of frequency to the exclusion of other conditions in learning is certainly more properly considered a feature of behaviorism than of associationistic thinking in general; the associationists have resisted stubbornly the attempts to eliminate such meaningful conditions as similarity and contiguity from the lists and laws. There is no prominent current theory which does not recognize the importance of incentive, set, or the determining tendency in some form.

However innocent the current associationistic theories may appear of the charges brought against them, it must nevertheless be admitted that the assumptions considered objectionable are often implicitly present in the experimental work. In the work on interference, frequency of repetition is generally accepted alone as an adequate measure of the effective strength of a habit or of the stage reached in the learning process. The prevalence of this practice may be judged by the frequency with which it occurs in the few researches cited above.

The positive suggestions of the *Gestalt* psychologists toward an approach to problems of learning (and specifically to problems of interference) are taken by the writers to be approximately as follows.[8] The primary determinants of reproduction are, first, the total organization of the conditions (notably incentives and set) under which the items occur together originally *plus* such changes as may occur in this organization during the interval before reproduction, and, secondly, the total organization of the conditions at the time of possible reproduction. If these two organizations are congruent, immediate reproduction will occur; if the two are dissimilar, reproduction will fail. Interference, if it is actually experienced or shown by errors in performance, is a result of conflicting stresses set up at the time of reproduction owing to the presence of two conflicting tendencies toward reproduction. To affect reproduction each of the tendencies must be a reproductive tendency. Association (by frequency of repetition) should refer to a mere mechanical condi-

[8] Köhler states his unwillingness to attempt the formulation of a complete theory of reproduction. The points mentioned here are those to which Köhler gives provisional acceptance.

tion by which the *strength,* but not the direction or character, of an organized reproductive tendency may be influenced. Hence, frequency of association can affect interference only indirectly by making the trace of the organized whole (reproductive tendency) more stable and enduring.

In support of such a view some evidence has been presented which has direct bearing upon our problem. Lewin performed experiments in which he demonstrated the relative unimportance of frequency of repetition.[9] When conditions were established during the practice period such as to encourage the 'wrong' reproductive attitude, decided interference occurred in the test period despite the small number (4 to 6) of repetitions. When practice conditions favored the proper reproductive attitude, there was no interference with the formation of a potentially conflicting association even though a great number (200 or more) of practice repetitions were given. Schwarz investigated interference by reversing one act in a complicated motor task.[10] By altering the difficulty of the other portions of the whole performance, he was able to regulate the *S*'s attitude (attentive spread). Schwarz found that direction of attention away from the crucial act greatly increased errors of reversion. Pauses for introspective report were also effective influences. Schwarz concluded that the organization of the total field is of primary importance in the determination of interference-effect.

On the side of theory, then, there are two directly opposed views as to the nature of interference. The researches which have given data upon our specific problem have for the most part followed in the associationistic tradition which leads to a formulation in terms of connections between items. A further point, not necessarily inherent in the associationistic approach but prominent in the experimental technique of these investigations, is the acceptance of frequency of repetitions as the sole determinant of the strength of connections and hence of their effectiveness in causing or resisting interference. This general approach, applied to our specific problem, holds promise of the existence of a constant relationship between amount of interference and stage of previous training and would logically imply that the relationship is more likely to be positive than inverse. The results obtained, however, are inconclusive. Although some relationship was found in all cases, Culler and Hunter found a positive relationship while Bair, Kline, and Wiltbank found evidence for a negative relationship. The anti-associationistic or *Gestalt* view suggests a formulation of the problems of interference in terms of conflicting reproductive tendencies or organizations each dependent upon conditions of the total situations. The character of these organizations depends not at all upon the amount of training; if

[9] K. Lewin, Das Problem der Willensmessung und das Grundgesetz der Assoziation, *Psychol. Forsch.,* 1, 1921, 191-302; 2, 1922, 65-140 (discussed by Köhler, *op. cit.*). Lewin takes as a point of departure the work of N. Ach, *Ueber den Willensakt und das Temperament,* 1910. Ach's view was that the strength of a determining tendency (reproductive) could be measured by placing it in opposition to an associative tendency. By increasing the number of repetitions the associative tendency could be strengthened until the interference was overcome. Ach, and others of the Würzburg school, seemed content to demonstrate the existence of other tendencies as well as that of association by frequency; the experimenters of the *Gestalt* school are intent upon showing that association by frequency is of minimal importance.

[10] G. Schwarz, Ueber die Rückfälligkeit bei Ungewöhnung, *Psychol. Forsch.,* 9, 1927, 86-158.

mere amount of training has any effect upon interference it must be the result of an influence upon the strength or stability of the organizations. Hence the *Gestalt* view holds little promise for the finding of any constant relationship between amount of interference and stage of previous training. The work of Lewin and Schwarz has demonstrated the effectiveness of other factors in overshadowing the influence of mere practice. However, even from the standpoint of this critical view, the problem under consideration assumes sensible importance if it can be dealt with in terms of actual stages of habit-formation, effective strengths of the organizations, rather than in terms of frequency of repetition.

The Experiment

The problem lends itself to a direct approach, that is, to the straightforward procedure of providing training in one task to any desired stage, then shifting the *S* to the learning of a second task and measuring the amount of interference in terms of the relative amount of difficulty encountered. In view of the apparent inadequacies of method found in the previous investigations, especial effort was made toward improvement of the experimental technique for this research along three lines: in the selection of the tasks, in definition and measurement of stages of training, and in provision of adequate measures of interference-effects. Telegraphic sending was selected as offering the greatest possibility for two conflicting tasks of practically equal difficulty (two forms of a single code) in which the factor of manipulative skill could be isolated and controlled. As the most exact and significant definition and measure for stages of habit-formation, actual proficiency was chosen. In the attempt to secure more adequate measurement of interference-effects, provision was made for comparing the course of improvement in the two tasks up to a definite stage of proficiency with respect to all of the various aspects of the process including time, errors, and total trials. Training in the second task was continued beyond the point of bare mastery, in order that interference-effects might be detected if they should persist or appear for the first time during the later stages.

Materials and Procedure

In the two forms of the code used the first 10 letters of the alphabet were represented by 10 combinations of dots and dashes. Toward securing equality in difficulty, the same 10 'dot-dash' combinations were used for both forms. A further check upon possible inequality of the two forms was provided by starting half of the *S*s in each group with one form and the other half with the other. Since the two forms were made up of the same 10 letters and the same 10 symbols, the incompatibility of the two tasks lay entirely in the pairing of letters and symbols.

The two forms of the code were arranged after many preliminary experiments. The length was fixed at 10 letters in order to allow for complete learning of one task within an experimental period. The symbols consisted of two, three, or four units (dots and dashes) each. Care was taken that neither of the symbols assigned to any letter should closely resemble the Morse symbol for that letter; a surprisingly high proportion of women college students admit having had some earlier acquaintance with conventional telegraph codes. For the purposes of the experiment it was desirable to find two forms which would give maximal interference, and much tedious experimentation was done upon this point. It was found that transfer rather than interference occurred if the two symbols for the same letter were similar either in number of units (*e.g. dot, dash* and *dash, dot*) or in general pattern (*dot, dash* and *dot, dash, dot, dash*). The symbols finally adopted for each letter differed in the two forms in both number of units and general pattern.

	Form X	Form Y
A =	*dot, dash, dash, dash*	*dash, dot, dash*
B =	*dash, dash*	*dot, dot, dash, dot*
C =	*dash, dot, dash*	*dot, dash*
D =	*dash, dot, dash, dot*	*dot, dash, dash*
E =	*dot, dash, dash*	*dash*
F =	*dot, dot, dot*	*dash, dot, dash, dot*
G =	*dash*	*dot, dash, dash, dash*
H =	*dot, dash*	*dash, dot, dot, dash*
I =	*dot, dot, dash, dot*	*dash, dash*
J =	*dash, dot, dot, dash*	*dot, dot, dot*

The *S* was seated at a table upon which a standard telegraph key was permanently mounted. For each trial she was required to tap out as rapidly as possible the 10 symbols for the 10 letters as arranged on a typed list. Whenever prompting was required, the *S* pressed a second key which revealed a complete chart of the code, both letters and their symbols. She was required to press this prompting key also whenever an error occurred. Both keys were operated with a single hand and were placed at a distance of about 10 in. apart. Each occasion of prompting or of error was thus represented within the single time record for the trial by an approximately uniform increment. An electric counter recorded the number of times the prompting key was pressed. The *E* presented the lists, timed the trials, and kept her records from a position behind a screen. Experimental periods were approximately an hour in length and for each *S* there was one period of experimenting on each successive day. The 20 *S*s were women students with the exception of 2 who were instructors. None of the *S*s who were used in preliminary experiments took part in the experiment proper.

Preliminary Training

In order to eliminate the influence of improvement in mere manipulative skill, improvement which would show up as transfer-effect, all *S*s were given intensive preliminary training in the sending (copying) of the dot-dash combinations to be used as symbols and in the manipulation of the prompting key. This training was continued until after improvement in speed had ceased and the *S* was apparently at her limit of motor speed.

The preliminary training followed the general procedure of the main experiment except for the fact that the lists presented to *S* were made up of the dot-dash combinations to be copied directly. Twenty-five practice lists were used, each having the symbols in different order. Some of the lists were placed in the prompting box so that the prompting key had to be pressed after the copying of each symbol. In order to motivate the *S*s the *E* informed them that the same dot-dash combinations were to be used as symbols in a code to be learned later. Training of the *S*s to what seemed their limits of improvement required from 60 to 120 repetitions. At the final speeds reached the *S*s required only from 8 to 10 sec. for copying each list.

For the purpose of detecting any further improvement in mere motor speed, which might occur during the course of the main experiment, provision was made for the copying of four practice lists during each of the later experimental periods. It was possible, therefore, to compare each *S*'s speed of sending during the learning of the second task with her speed during practice in the first task. Comparison of figures for the last four lists in the preliminary training with those for the four lists on the last day of practice in the second task, that is, comparison of speed at the beginning and at the end of the experiment proper, showed only slight differences. Five *S*s lost slightly in speed, 10 improved slightly, and 5 showed absolutely no change. Since the results are treated later in terms of the differently trained groups, comparison here is more significant in those terms. In group average time for a trial the Half-trained Group improved 1.3%, the Barely-trained Group 0.2%, the More-trained Group 4.9%, and the Most-trained Group 6.2%. Mere manipulative skill improved only very slightly during the experiment, and the order and direction of the changes in motor skill are such that they could have had no significant influence upon the large differences between groups shown in the experiment proper.

Training in the First Task

On the basis of experience gained in preliminary experiments, four stages of training in the first task were selected and were defined in advance in terms of proficiency. The *S*s were divided into 4 groups of 5 each.

Groups with brief training:

(1) Half-trained. Training stopped as soon as five of the ten symbols were correctly produced in a single trial. (The designation for this group is not to be taken literally.)

(2) Barely-trained. Training stopped when three errorless trials occurred in succession.

Over-trained groups:

(3) More-trained. Training continued through the first experimental period in which errors were consistently absent and in which the speed remained practically constant throughout all trials.

(4) Most-trained. Training continued until there had been no errors and no appreciable improvement in speed for at least three days.

Although the criteria for the over-trained groups appear somewhat indefinite it was possible in actual practice to distinguish such stages with a fair amount of definiteness. For the More-trained Group the *Ss* arrived at a stage where for the first time they could with considerable effort avoid errors and maintain consistent speed throughout a whole period. For the Most-trained Group the task had become absolutely automatic in its performance so that each new trial was for the *S* a matter of routine.

The general method used in learning the code has already been described. At the beginning of the first period 20 trials of copying the symbols were given in order that this period might correspond more closely to the initial period of learning of the second task where the shift would occur after a number of trials of the first task. At the beginning of the actual training the *S* was told, along with the explanation of her task, that she should strive for speed and hence should dispense with the use of the prompting key just as soon as possible. The time for each trial was announced at its completion. An interval of about 10 sec. was required between trials for recording, and a rest period of one minute was given after every 10 trials. A point was made of engaging the *S* in general conversation during these rest periods to prevent thinking over the code and engaging in implicit practice. Forty lists of the letters in different orders were used, and the order in which the lists were presented was changed daily. The Half-trained and Barely-trained Groups completed their training during a single experimental period. The two over-trained groups practiced on the first day until 3 successive errorless trials were achieved; their training was continued on the following days at the rate of 40 trials each period until the desired proficiency was reached.

The number of trials required by individuals to reach the defined proficiency for the group varied tremendously. For the Half-trained Group the range was from 8 to 18 trials, for the Barely-trained from 17 to 39, for the More-trained from 83 to 120, and for the Most-trained from 260 to 460. These figures illustrate clearly the impossibility of defining stage of training in terms of repetitions. The group averages in number of trials required and final time for a trial were:

	Av. Number of Trials	Av. Final Time	Total Improvement
Half-trained	12.4	25.1 sec.	40%
Barely-trained	26.1	15.8 sec.	60%
More-trained	95.6	11.9 sec.	70%
Most-trained	308.0	8.1 sec.	80%

The approximate percentages of total improvement in speed were obtained by comparing the times for the first and the last trials of this task; the times for the first trial were of the order of 40 sec. The figures show that it was necessary to add each time an increased number of repetitions in order to secure progressively decreasing increments of improvement in speed, as would be expected from the shape of the typical learning curve. The four values for the final time of each series indicate that the groups were shifted from this task at stages which were distinctly different as was indicated also by the error criteria incorporated in the definitions of the stages.

Learning of the Second Task

The shift from the first to the second task occurred always in the middle of an experimental period, and the interval between the last trial of the first task and the first trial of the second was uniformly 3 min. This arrangement and interval were chosen because of some evidence which would indicate that the smaller the time interval between tasks the greater will be the interference. The method used for the second task was exactly the same as that for the original task. The learning extended over three periods. On the first day the shift to the new task occurred after 10–30 repetitions of the first task, and practice was continued until 3 successive errorless trials were achieved. On the second day there were 40 repetitions and on the third day 20.

The *Ss* were instructed to use exactly the same method in learning the new code as that used for the first task. Preliminary experiments had shown a wide difference among *Ss* as to the manner of approaching a new task. Some *Ss* were constantly reminded by the letters of their previous symbols and involuntarily related each symbol to both its new letter and the one formerly represented. Other *Ss* were never reminded of previous connections and practically failed to recognize the symbols; they accepted fully the notion that they were starting an entirely new task. The possible effects which these two different attitudes might have upon interference were not determined in our brief preliminary experiments, but the point is certainly one well worth separate investigation. In the attempt to secure as much uniformity as possible the *Ss* were told that they were absolutely through with the old code and must now put it completely out of mind. As was shown by the introspective reports this attempt to induce voluntary control was only partially successful even at the beginning. However, it did serve to prevent the mistake on the part of the *Ss* of accepting the new task as one involving the relating of two codes.

Interference During Initial Learning of the Second Task

In the treatment of the results the basic method used is that of comparing the learning to a certain stage of proficiency in the second task with the original learning to the same stage in the first task. Greater difficulty in the learning of the second task is taken to indicate interference-effect, and greater facility with the second task is taken to indicate transfer-effect. Comparisons between the differently trained groups are based upon the relative size and direction of these effects.

For a first comparison a convenient and significant stage is that of bare mastery, the stage at which the first errorless performance occurs. Results obtained for the learning of each task to this stage of proficiency are given in Table I.

These results indicate, in general, interference-effect for the two briefly-trained groups and transfer-effect for the overtrained groups. Differences

in difficulty might show up as differences in any one, or in any combination, of three variables: number of trials, number of errors, and bare speed for each trial. The time record was designed to reflect both bare speed and delay for correction of errors, so that the time for a trial as recorded is in part a function of the number of errors. As the results come out, the average time for a trial differs very little between tasks and shows in the second task only very slight differences between groups; for this particular experimental arrangement the differences in difficulty show up primarily in the number of trials required. Since the total time reflects both the number of trials and the time for each trial, it is a more adequate single measure

TABLE I

RESULTS FOR LEARNING IN EACH TASK TO THE POINT OF BARE MASTERY

The signs indicate the direction of the differences; 'plus' indicates transfer-effect, 'minus' interference-effect. For the Half-trained Group training in Task A stopped at 'half-mastery;' the figures supplied in parentheses are averages for the 10 Ss of the Barely-trained and the Most-trained groups. These two groups were selected because of close similarity between their curves and that for the Half-trained Group up to the point where the latter stopped.

		Most-tr. Group	More-tr. Group	Barely-tr. Group	Half-tr. Group
Av. No. of Trials	Task A.....	26	29	26	(26)
	Task B.....	18	27	29	24
	% diff.....	+30.8	+6.9	−11.5	+7.7
Av. No. of Errors	Task A.....	127	143	138	(132)
	Task B.....	101	135	147	133
	% diff.....	+20.1	+5.6	−6.5	−0.4
Av. Total Time	Task A.....	698	894	672	(685)
	Task B.....	584	823	755	690
	% diff.....	+16.3	+7.9	−12.3	−0.7
Av. Time per Trial	Task A.....	28.8	30.8	25.8	(27.3)
	Task B.....	30.4	30.5	26.0	28.7
	% diff.....	−5.5	+1.0	−0.7	−5.1

than any of the former. According to the total time required, the Half-trained Group shows very little effect, the Barely-trained Group shows definite interference, the More-trained Group shows definite transfer, and the Most-trained Group shows the greatest amount of transfer.

The percentages and directions of difference between the results for the two tasks are presented graphically in Fig. 1.[11] Comparison of the effects shown for the different groups leads to the conclusion that in general there is an inverse relationship between amount of interference and amount of previous training. The results for the Half-time Group, however, sug-

[11] For comparable figures from other researches see Wiltbank, *op. cit.*, 42, and Hunter, *op. cit.*, 40.

gest a modification to the effect that the inverse relationship holds only beyond a certain moderate amount of training. Bair and Wiltbank found this inverse relationship, and Kline suggested the modification to the rule.

The results have been treated up to this point in relation to gross differences spread over a large segment of the learning curves. In the hope of

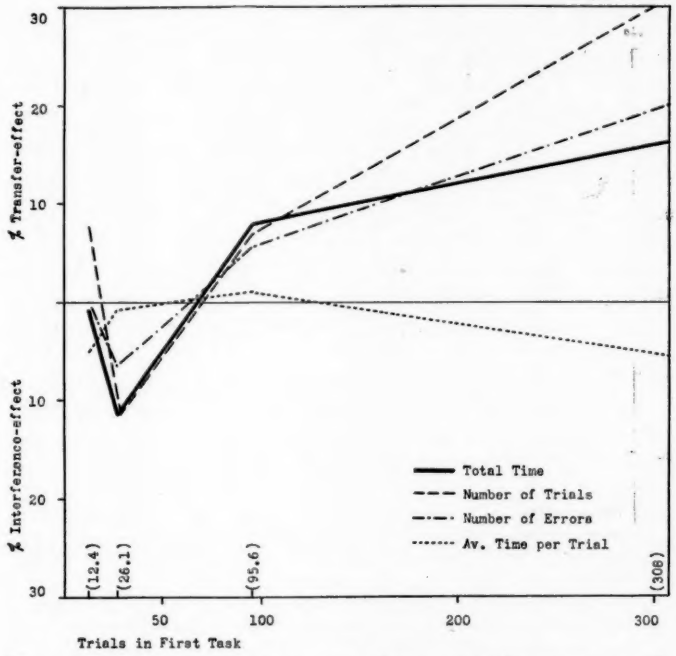

FIG. 1. PERCENTAGES OF TRANSFER- AND INTERFERENCE-EFFECT IN THE BARE MASTERY OF THE SECOND TASK PLOTTED AGAINST THE NUMBER OF PRACTICE TRIALS GIVEN IN THE FIRST TASK

Figures just above the base line indicate average trials for each of the four stages of training provided.

locating and tracing the occurrence of the effects more precisely, complete learning curves for each of the tasks were plotted for all *Ss*. It would seem a simple matter to compare the curves and to detect interference at the points where the second curve falls above the first. However, for these curves the most striking differences were in length and not in height so that direct vertical comparison could be of little significance. Moreover,

because of the differences in length it was impossible to combine individual curves into curves for group averages. These difficulties were overcome by conversion of the raw curves into Vincent curves.[12] The total number of trials was first divided into tenths and the time required for each tenth was then found. The Vincent curves based upon group average figures are shown in Fig. 2.

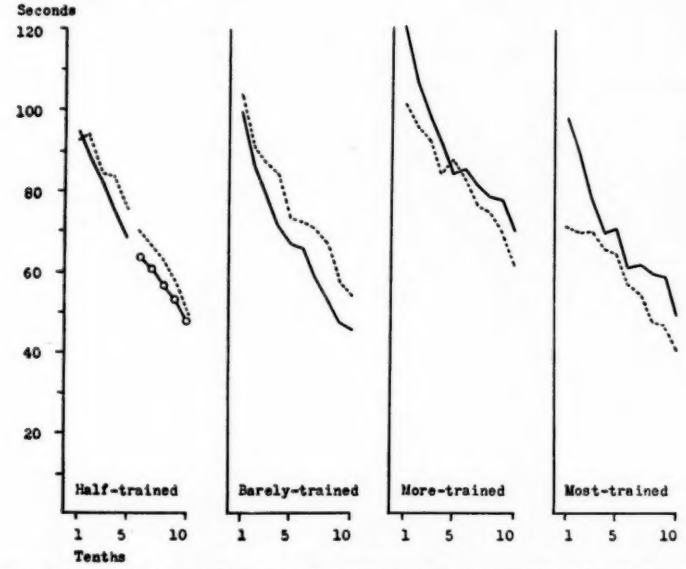

FIG. 2. VINCENT CURVES SHOWING THE AMOUNT OF TIME SPENT IN EACH TENTH OF THE TOTAL NUMBER OF TRIALS REQUIRED FOR BARE MASTERY OF THE TWO TASKS

Solid lines represent the first task and broken lines the second. The continuation of the solid curve for the Half-trained Group is based upon averages for the ten subjects of the Barely-trained and the Most-trained Groups. Figures for the More-trained Group were not included in these averages because of the obvious dissimilarity to the other three groups in level of performance.

The irregularity of the curves of Fig. 2 makes interpretation somewhat difficult. However, some general tendencies are rather clearly shown. For the two over-trained groups the curves indicate general transfer-effect, great at the beginning and end and less in the middle portion. For the

[12] For discussion of Vincent curves, see S. B. Vincent, *Behav. Monog.*, 1, 1912, (no. 5), 16-17; also, W. S. Hunter, in *Foundations of Experimental Psychology*, 1929, 567-68.

Barely-trained Group interference-effect is shown throughout, increasing in amount from beginning to end. For the Half-trained Group interference-effect is indicated in the middle portion.

The fact that the curves for the second task, with the exception of the curve for the Barely-trained Group, are generally concave to the base is partly explainable by the manner in which learning started. For a number of trials at the beginning it was necessary to look up each symbol so that the task was really a mechanical one. Although the *Ss* were instructed to try to avoid use of the prompting key as soon as possible, the introspective reports show that during the early stages many *Ss* found it more speedy to press the key than to "wait for memory of the symbol to appear." However, this same situation was reported at the beginning of the first task where conditions were exactly the same. In the case of the second task one may assume, with general support from the introspective reports, that the symbols "appeared in memory" more slowly, so that *S* resorted to the use of the prompting key to a greater extent.

It is obviously not safe to draw detailed conclusions from comparison of these curves until their further course has been traced. However, comparison to this point does indicate that the effects vary during the course of training in the second task and that the manner of variation differs between groups. There is definite suggestion that the relative positions of the groups with respect to amount of interference-effect shown may depend upon the point at which the measurements are taken. In line with this suggestion the various measures were applied at the stage of 'half-mastery' (5 correct symbols), and percentages of transfer- or interference-effect were calculated. These percentages of difference between the learning of the two tasks to the point of 'half-mastery' are given in Table II. 'Plus' indicates transfer-effect and 'minus' interference-effect, as in the first table.

TABLE II

PERCENTAGES OF DIFFERENCE BETWEEN THE LEARNING OF THE TWO TASKS TO THE POINT OF HALF-MASTERY

	Most-tr. Group	More-tr. Group	Barely-tr. Group	Half-tr. Group
Av. No. of Trials, % diff.	+8.3	+3.7	+3.2	−8.3
Av. No. of Errors, % diff.	+11.5	+5.2	−1.0	−12.5
Av. Total Time, % diff.	+0.2	+7.8	+0.4	−6.1

The principal difference between the effects indicated here and those shown by measurements to the point of bare mastery (Table I and Fig. 1) lies in the reversal of the positions of the two briefly-trained groups with respect to the greatest amount of interference-effect. Here at the stage of 'half-mastery' the Half-trained Group shows definitely the most interference, and the Barely-trained Group shows little effect in either direc-

tion. At this stage the results for these two groups fall into line with the general rule of inverse relationship between interference and previous training, while at the stage of bare mastery they seemed to constitute an exception to the rule. Such a discrepancy between two sets of measurements taken at different early stages points to the importance of tracing the course of changes in the effects through further stages in the learning.

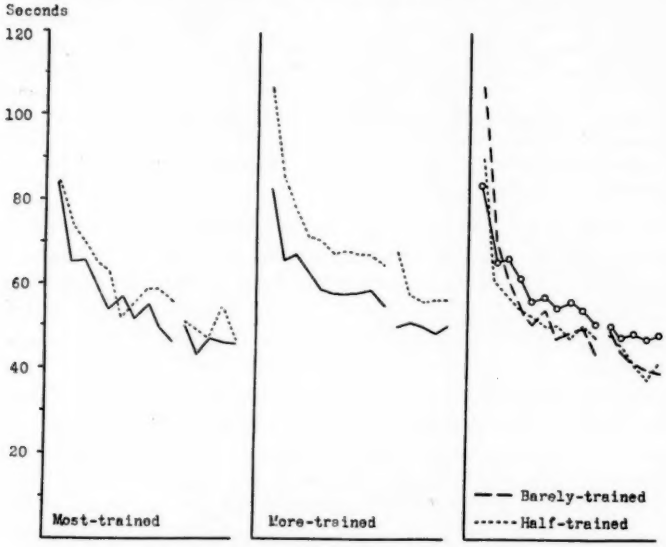

FIG. 3. CURVES SHOWING THE AMOUNT OF TIME SPENT IN EACH FOUR TRIALS DURING TRAINING BEYOND THE STAGE OF BARE MASTERY

Solid curves are for the first task, broken curves for the second. The solid curve in the case of the briefly-trained groups is based upon averages for the ten subjects of the over-trained groups. The gap in all curves indicates the interval between the two days.

INTERFERENCE DURING CONTINUED TRAINING IN THE SECOND TASK

Training in the second task was continued for two days after the stage of bare mastery had been reached. For consistency with the rest of the experiment it would have been desirable to define the length of this additional training in terms of arrival at a certain stage of proficiency. However, with the stage of errorless performance already passed, it was difficult to set a further standard in terms of errors, and, with the great inherent differences in speed, it was impractical to set in advance a standard in such

terms. Hence the number of trials was taken as the only possible means for defining the length of the additional periods. Forty trials were given on the first day and 20 on the last day. Figures for comparable periods of training in the first task were available for the two over-trained groups. With the number of trials made constant and with errors practically absent, the total time is here a simple function of the time for each trial. Curves showing the amount of time spent in each 4 trials during the two periods beyond bare mastery are shown in Fig. 3.

These curves for the later stages of training seem to indicate interference-effect for the two over-trained groups and transfer-effect for the two briefly-trained groups. In the case of every group the relation of the second curve to the first is the reverse of that found during the period preceding the stage of bare mastery (Fig. 2). Between interference and previous training a positive relationship is shown at these later stages instead of the inverse relationship shown in the measurements at earlier stages.

Obviously the curves of Fig. 3 are not directly comparable to those of Fig. 2 where differences in height represent chiefly translated differences in the number of trials. In order that the two sets of results may be treated together, the figures for the later stages must be brought into relation to some defined stage of proficiency. If the average speed for each group in the first 5 trials of the first task is taken as the original speed of performance in this activity, percentages of total improvement can be found on this basis. Calculated to the average time for the last 5 trials on the last day these percentages of improvement are:

	First Task	Second Task
Most-trained Group	67.8	67.8
More-trained Group	64.4	64.6
Barely-trained Group	—	71.5
Half-trained Group	—	74.9

While these figures show that for the two over-trained groups the stages of proficiency reached were closely similar, there is the indication that the two briefly-trained groups were carried beyond in improvement. It is now possible to set 60% as a standard percentage of improvement in speed to be effected and to go back to the raw learning curves for the points at which this standard was attained. The figures obtained by this treatment of the results are given in Table III.

These figures indicate again interference for the over-trained groups and transfer for the briefly-trained groups when the measurements apply to learning to a relatively advanced stage. There is a definite tendency for the

relations between groups to be roughly the reverse of what they were when measured at or before bare mastery. Although the calculations of percentages of effect for the two briefly-trained groups are based upon mere averages of the other *Ss*, direct comparison of the figures for total time in the second task shows that here in Table III the figures for the two briefly-

TABLE III

RESULTS FOR LEARNING TO THE POINT WHERE THE AVERAGE TIME PER TRIAL IS 40% OF THE AVERAGE TIME FOR THE FIRST 5 TRIALS OF THE FIRST TASK

Results to this stage in the first task were not available for the two briefly-trained groups figures supplied in parentheses are averages for the 10 *Ss* of the other two groups. The 'plus sign before percentages of difference indicates transfer-effect and the 'minus' sign indicates interference-effect.

		Most-tr. Group	More-tr. Group	Barely-tr. Group	Half-tr. Group
Av. No. of Trials	Task A.....	42	45	(43)	(43)
	Task B.....	42	67	45	32
	% diff.....	0	−48.8	−3.5	+26.4
Av. Total Time	Task A.....	974	1174	(1074)	(1074)
	Task B.....	994	1573	1051	842
	% diff.....	−2.1	−33.9	+2.2	+21.7

trained groups are disproportionately low in comparison to their position with respect to the other groups in Table I.

These comparisons are still subject to one limitation: proficiency at the early stages was defined in terms of errors and at the later stages we have sought to define it in terms of speed. This shift in criterion may be justified logically by the fact that at the early stages the principal aim of the *Ss* was improvement with regard to errors while at the later stages, with errors already eliminated, the aim was improvement in speed. However, a more objective justification was sought by treating the results of the first day of training in terms of the achievement of a stated percentage of improvement in speed. Percentages of difference between the two tasks in total time were calculated to the point of 20% of improvement in speed over the original average speed for 5 trials of the first task. The two briefly-trained groups showed large differences toward interference-effect: 44.7% for the Barely-trained Group, and 36.6% for the Half-trained Group (calculated only to the point of 12% of improvement). A difference of 3.1% toward transfer is shown in the case of the More-trained Group. The figures for the Most-trained Group (with the exclusion of one *S* who did not improve in speed for either task during the first day, although she quickly eliminated errors) show some interference (about 18%). Comparison of the relationships between groups here with those at the later stage (both stages defined in terms of improvment in speed) lends support to our general conclusion. When measurement is shifted to a later stage, there is a real shift, roughly a reversal, in the arrangement of the groups in order of amounts of interference.

Fig. 4 shows in composite form the results obtained by measuring the effects at three different stages in the learning of the second task.

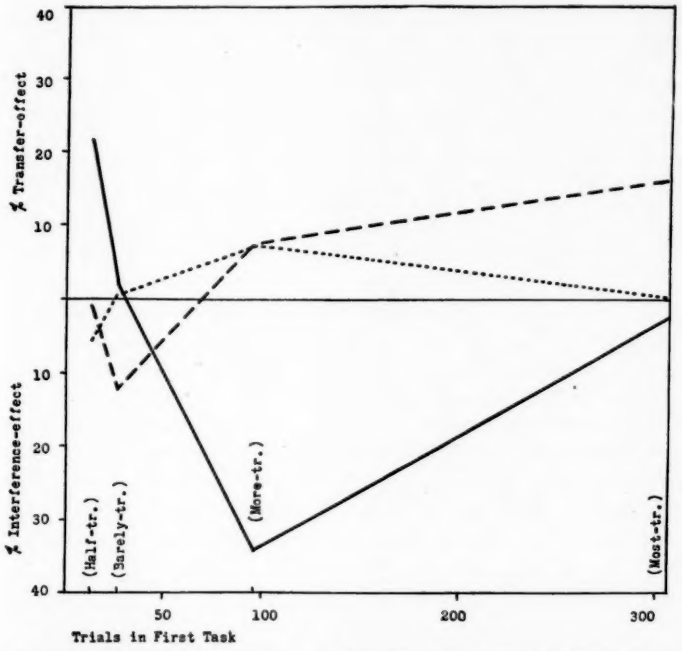

FIG. 4. PERCENTAGES OF TRANSFER- AND INTERFERENCE-EFFECT IN TERMS OF GAIN OR LOSS IN TOTAL TIME REQUIRED, PLOTTED AGAINST THE NUMBER OF PRACTICE TRIALS GIVEN IN THE FIRST TASK

The three curves show the results of measurements taken at three different stages of proficiency in the second task: 'half-mastery' (line of dots), bare mastery (line of dashes), and improvement of 60% in speed (solid line).

DISCUSSION OF RESULTS

The point of outstanding significance in the results is the demonstration of the manner in which the amount of interference, resulting from a given amount of previous training, changes during the successive stages of training in the second task. The time at which the interference will occur, as well as its amount, is determined by the stage of previous training. Hence, the question of which stage of training produces the most interference can be answered adequately only if measurements at several stages are taken into consideration. During early stages of training in the second task the greatest interference-effect is shown by the groups with brief training in the previous task; during later stages the greatest interference-effect is shown by the over-trained groups.

In the light of these findings it is easy to see how different investigations might lead to conflicting results if the measurements were made in each case at only a single stage. An examination of the conditions provided in the previous researches which have been cited suggests that the differences in results are due, at least in part, to differences in the stages selected for measurement. Insofar as may be judged from the number of trials and the nature of the tasks, it seems generally true that the investigators who found an inverse relationship between interference and previous training measured at relatively early stages, while those who found a positive relationship measured at advanced stages. While our findings can not be said to offer a basis for resolving all conflict between previous sets of evidence, they do point out another important way in which these previous results are unsuited for direct intercomparison.

The changes in amount and direction which the effects undergo during training in the second task seem to follow in each case a definite rule. The order in which the different groups arrive at their respective points of maximal interference-effect is precisely parallel to the order in which they shifted from training in the first task. The first three groups show maximal interference-effects at stages which correspond directly to the stages reached in the first task. This fact is brought out strikingly in Fig. 4 where the lowest point on each curve is that for the group which was trained in the first task to the same stage at which interference was measured in the second task.

The curves of Fig. 4, and our measurements in general, do not truly represent the extent of the effects at a particular stage; they represent the cumulative effects throughout the whole learning process up to a given stage. When, for example, a small degree of transfer-effect is shown for the Barely-trained Group at the stage of 60% improvement, this really means that just before this point of measurement there was a great amount of transfer-effect sufficient to cancel out the previous balance toward interference-effect and still leave a net gain for the whole period. At the stage of the last measurement the Most-trained Group was experiencing an increasing amount of interference which had already had sufficient effect to cancel out the large transfer-effect shown up to the stage of bare mastery. It seems natural to assume that this group would have reached its maximum of interference-effect at a later stage had the training been continued to a stage corresponding to that reached in the first task. This assumption gains in plausibility from the fact that the maxima for the first three groups show, in the order in which they occur, a progressive increase in size, while up to the stage of the last measurement the Most-trained Group has shown no interference-effect as great as even the smallest of those for the other three groups.

The evidence from the introspective reports is in complete accord with the formulation that interference occurs maximally when training in the second task is at the stage which was reached in the first task. All *Ss* described interference as definitely present at some time, and when the

effect was at its height many actually ascribed their difficulties to the similarity between their present stage of performance and that at which they had left off in the previous task.

The manner in which the interference was experienced by the *Ss* is of interest here. No questions were asked by the *E*, and the reports were purely spontaneous on the part of the *Ss* except for an occasional request that some point mentioned be elaborated. Almost without exception the *Ss* reported that, when difficulty was experienced, it could be of either one of two distinct types. The first type, usually considered less 'serious' than the other, was described as the occurrence of a complete blank space in consciousness accompanied by inability to send any symbol. This type of blocking occurred quite suddenly—several letters of a list would be sent smoothly and the next letter would then produce a "mental vacuum." Finally the correct symbol would "appear" or the prompting key would be remembered. No suggestion of the first code entered consciousness, nor was there any tendency toward reversion either at the time of the blocking or later. As soon as the blocked condition was displaced, the *S* could go on sending the correct symbols as smoothly as ever. Although this type of difficulty involved an immediate loss of time, it was not regarded as at all serious, since it did not affect the ease or the speed of sending the rest of the symbols.

The second type of difficulty was described as direct intrusion by the symbols of the first code. Just as she started to send a symbol, the *S* would suddenly stop and say, "No, that is the old code." Once this had happened it was impossible for the *S* to keep the old symbols from coming to mind for as many as several trials or for even a whole period. The *Ss* regarded such periods, with considerable annoyance, as periods of truly serious difficulty. During such periods the *Ss* complained of the fact that, when one is equally familiar with two different sets of symbols, it is impossible to keep the two separate. Those who experienced such difficulty only at a late stage remarked that the separation had been easy before because the old task had become a routine matter of sending symbols automatically at high speed and so was completely dissimilar to the slow, effortful performance at the early stages of the second task.

It would seem, then, that the effective type of interference, the type which shows up plainly in the objective measures, involves a true reversion to the 'set' or attitude of the former task. Such a reversion is likely to occur only when the two tasks are at such stages that the general modes of approach, in this case the 'motor set' for a certain speed and the general direction or level of attention, are the same for both performances. When the two tasks are at dissimilar stages, an overt reversion may occur and be corrected without being recognized and will cause no further disturbance. When the two tasks are at similar stages, a reversion will at once bring up the whole previous set and will introduce a period of great difficulty. It is even possible, as is shown in several instances, for an overt reversion to occur and to be recognized without further consequences if the two tasks are at the time regarded as dissimilar. In such a case the *S* dismisses the matter

with a remark such as, "My hand got ahead of me," and is able to settle back at once to the present task at its own slow pace.[13]

In view of the theoretical background of the problem the question arises as to whether the difficulty experienced during interference was primarily a matter of difficulty in association or in recall. While the separation of these two processes seems a somewhat artificial one, the introspective reports do indicate that the *S*s localized the difficulty in the process of recall (or reproduction). If interference is to be regarded as a conflict between attitudes or tendencies, the tendencies must be reproductive rather than associative.

A typical report of one *S* will serve to illustrate this point. At the time of great interference-effect, she said: "I try to think of the symbol or to send it, but it just will not come; the wrong one keeps getting in the way. Then I look at the prompting code, and there is the symbol. It belongs to the letter so much that I feel foolish at forgetting it, but a few minutes later I can't make it come when I need it."

The answer, then, to our original question of just which stage of training results in the most interference must begin with the provision that it depends upon the stage at which interference is measured. If measurements are taken at various stages and if we compare the maximal effects for the different groups regardless of the stage of occurrence, our results point to a positive correspondence between amounts of interference and amounts of previous training. Within the range of our measurements there is a progressive increase in the amount of interference shown for three groups in order of increased training. Unfortunately our measurements did not extend to the stage at which maximal effect for the Most-trained Group could be expected, but there is reasonable ground for assuming, both from the objective and the introspective evidence, that the effect for this group would have followed in the progression.

Despite the satisfyingness of such a generalization, critical consideration shows how little it can really mean. Although the amounts can be compared, the essential nature and the practical effect of the interference is different for each case. In the case of the briefly-trained groups the interference consists in difficulty at the initial, slow stages of performance; the difficulty is soon overcome and further learning progresses smoothly. With the over-trained groups there is no difficulty in performing the new task and in learning it to a certain stage; the difficulty here is a matter

[13] Under the present experimental conditions, overt reversion to the extent of the actual sending of the former symbol occurred very infrequently. For all *S*s during the entire experiment only 92 instances were recorded. The number of such reversions has no apparent relation to the amount of interference shown according to other criteria. The fact that the significance of reversion may differ in different instances suggests possible inadequacy in an experimental technique which employs only motor reversion as the direct measure of interference.

of speed, an inability to progress beyond a certain relatively advanced level. For a problem in which learning of a second task was required only to the point of bare familiarity, the results of previous overlearning in a conflicting task could have little or no significance, although the magnitude of the effect of such over-learning is in terms of the generalization very great.

In introducing the problem and its history we found it necessary to refer to two opposed theoretical views, the view of the *Gestalt* psychologists and the view which is called by them associationistic. In general our results are much more amenable to treatment in the terminology of *Gestalt* than in terms of associationistic connections. However, under the conditions of this experiment we did find that the stage of habit-formation, the 'mere strength' of the organization, can be a determinant of interference both as to its degree of effectiveness and as to the time of its occurrence. The conditions of the experiment were not sufficiently different from those under which motor learning goes on in everyday life to lead to agreement with the general proposal of the *Gestalt* psychologists to the effect that ordinarily mere strength of a habit is a negligible factor in influencing effective reproduction.[14]

SUMMARY

(1) Interference-effects appear at different stages in the learning of a second, inconsistent task, dependent upon the stage of training reached in the first task. For a given stage of original training the maximal effect will appear when the learning of the second task reaches a stage approximately the same as that finally reached during training in the first task.

(2) If interference-effect is measured during the initial stages of training, an inverse relationship between the amount of interference and the stage of previous training is found. If measured at more advanced stages of training, the amounts of interference bear a direct, positive relationship to the stages of previous training. If the maximal amounts of effect are compared without regard for their different points of occurrence, a positive relationship to the amounts of previous training is indicated.

[14] Since the acceptance of this article for publication, J. A. McGeoch has published results of studies on the influence of degree of interpolated learning upon retroactive inhibition (this JOURNAL, 44, 1932, 695-708). An analysis of McGeoch's results reveals a tendency for the maximal retroactive effect to occur when the interpolated task has been learned to about the same degree as the original task. This suggests the possibility that the general law indicated by the present study of interference may apply also to retroactive inhibition. Experiments designed to test this possibility are now in progress.

AN IMPORTANT FACTOR IN SPACE PERCEPTION IN THE PERIPHERAL FIELD OF VISION

By C. E. FERREE, G. RAND, and C. HARDY, Johns Hopkins University

Two important reasons may be given for the poor space perception in the peripheral field of vision; defective imagery and a poorly developed sensorium. With reference to the relative importance of these reasons or factors, the concensus of opinion seems to incline to the latter. In a recent paper,[1] the belief is expressed that the refractive condition in the peripheral field can not play an important rôle because refractive errors in central vision do not seem to have very much effect on the space perception in the peripheral field. In this opinion the fact seems to be overlooked that as compared with central errors of refraction, the errors in the peripheral field are so great that not much effect could be expected. They modify the type of error found in the peripheral field, it is true, but clearness of imagery depends upon the amount rather than the type of defect.

It has been the purpose of the present experiments to study the refractive conditions in the peripheral field. One of the reasons for making this study is the bearing of these conditions on the explanation of the peculiarities and anomalies of peripheral space perception. Other important incentives have been their relation to acuity in the peripheral field; to achromatic and chromatic sensitivity in peripheral vision and to the limits of the form and color fields; their possible relation to defects of imagery in the central field and to ocular deviations; and the information which they give as to the position of the lens and the symmetry of conformation of the retina. In a later paper results will be given which bear still more directly on the importance of the refractive conditions in the peripheral field as a factor in peripheral space perception.

With reference to the above needs or any needs of practical application, the attempts that have been previously made to refract the eye for the peripheral field are unsatisfactory for the following reasons: (a) the methods used were either unsatisfactory as to accuracy or so cumbersome as to ap-

* Accepted for publication November 29, 1931. From the Research Laboratory of Physiological Optics, Wilmer Ophthalmological Institute, Johns Hopkins Medical School.

[1] M. J. Zigler, B. Cook, D. Miller, and L. Wemple, The perception of form in peripheral vision, this JOURNAL, 42, 1930, 246-259.

paratus and procedure as to be worthless for practical work; (b) the results were not expressed in a form that has practical meaning or value; and (c) the determinations extended but a short distance into the peripheral field. A very important part of our purpose in making the study reported in this paper has been, therefore, the development of a method that will permit of determinations in the more remote parts of the field, that will be reasonably satisfactory as to accuracy and feasibility, and that will give results directly in terms of diopter values.

The two most important factors in the ametropia for the peripheral field are (1) the effect of the oblique incidence of the rays of light from objects in the peripheral field on the clearness of the image formed; and (2) the effect of size and shape of the eyeball and of anomalies and irregularities in the conformation of the retina on the distance of the percipient elements from the nodal point of the refracting system. With reference to the first of these factors it will be remembered that clear images are formed by lenses only when the object to be imaged is located on the principal axis of the lens. When the object is displaced from the principal axis a distortion of the image occurs which varies in amount with the angle of displacement of the object or with the angle of incidence of the light on the lens. In general, the effect of varying the angle of incidence is that of adding a weak plus sphere and a strong plus cylinder with its axis at right angles to the plane of incidence. The major effect is thus to create a strong astigmatism. A simple formula for this effect in the two meridians may be expressed as follows:[2]

$$F_1 = F(\mu - 1)/(\mu \cos b - \cos a)$$
$$F_2 = F_1 \cos^2 a$$

In these formulas F represents the focal length of the lens in question; F_1 the focal length in the meridian of least refractive power; F_2, the focal length in the meridian of greatest refractive power; a, the angle of incidence of the light; b, the angle of refraction at the first surface; and μ, the refractive index of the lens. In both meridians, then, the image is brought nearer to the lens as the angle of incidence increases, very much nearer in the meridian of the oblique incidence and very little nearer in the meridian at right angles to it. The refractive effect for the eye would thus be a slight myopia compounded with a strong myopic astigmatism. The effect on vision is modified, however, by the fact that the distance of the retina from the nodal point of the refracting system decreases rapidly as the distance from

[2] L. Laurance, *General and Practical Optics*, 1908, 163-166; 3rd ed., 1920, 245-247.

the fovea is increased. In the greater number of eyes the effect of this is to bring the retina between the foci in the two meridians or to cause a mixed astigmatism which increases in diopter value as the periphery of the retina is approached. As will be shown later in the paper, however, other types of eye are also found, but they do not seem to be of such frequent occurrence.

Method and Apparatus

For the refraction of the eye for the peripheral field a Zeiss parallex refractionometer modified to suit our purpose was used. Like all the objective methods of refraction, the method used with this instrument provides a means of locating the point or plane which is the anterior of the pair of conjugate foci of the refracting system of the eye. Of this pair the fundus is the posterior focus. Also like the other objective methods—refraction with the ophthalmoscope and with the retinoscope—the method is only approximately correct in principle, *i.e.* the fundus and not the percipient elements is taken as the posterior focus. The reflecting surfaces which make up the fundus cannot be assumed to have the same location as the percipient elements nor even to sustain an invariable relation to them in the different parts of the eye ground and in different eyes. The distance between these reflecting surfaces and the percipient elements is minute, it is true, but in a refracting system as powerful as that of the eye even minute changes of distance in the posterior focus may become of consequence in the location of the anterior focus. The subjective methods of refraction alone start with the true posterior focus of the refracting system. This is one of the reasons why they should be the final court of appeal in making an accurate refraction.

The subjective method, however, we find, can be used with but indifferent success in the peripheral field. Of the objective methods, that used with the parallex refractionometer has thus far been found by us to be the most satisfactory for this work.

In brief, the principle of locating the anterior of the conjugate foci of the refracting system of the eye utilized in the Zeiss refractionometer is as follows. At the principal focus of the objective lens of the instrument is placed a transilluminated test object. The rays of light from this test object are collimated by the lens and enter the pupil of the eye as a parallel beam. If the eye is emmetropic the image will be focused on the fundus (approximately). Likewise when the eye is emmetropic the light reflected from the fundus will emerge from the pupil of the eye as a parallel beam and will be focused by the objective lens of the instrument at the position of the test object. In order to be able to compare the distances of the test object and of the image so formed from the objective lens of the instrument, the principle of parallax is used; that is, the line of view of the examiner is displaced slightly to one side of the principal axis of the refracting system of the eye and objective lens. If the distances of object and image from the objective lens are equal, the image will superimpose on the test object and will not be visible (emmetropia). If it is nearer to the lens than the test object, the image will be seen displaced to one side of the test object, the side towards the illuminating tube of the instrument (myopia); if it is further from the lens than the test object, it will be seen displaced to the other side, the side opposite the illuminating

tube of the instrument (hyperopia). In order to provide for the refraction of the eye in different planes in the examination for astigmatism, the system can be rotated through 180° and the determinations made at any position within this range. In case the position of the test object and of its image are found not to coincide, the test object is moved until exact coincidence is obtained. The vertex refraction can then be read from a scale which is seen just above the test object. For a more detailed description of the instrument, the reader is referred to the circular issued by the manufacturer.

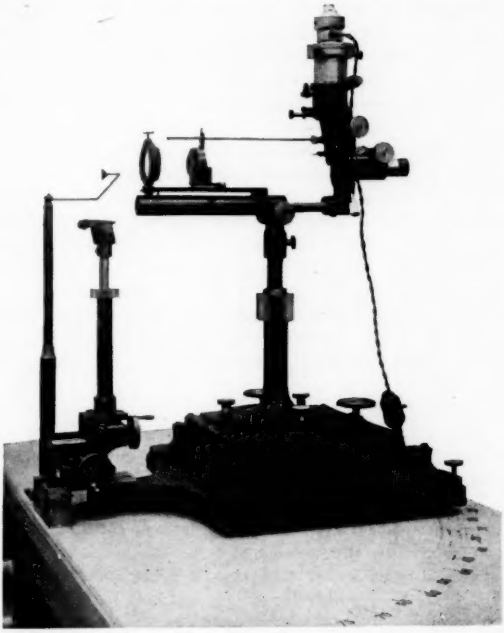

FIG. 1. MODIFIED ZEISS PARALLAX REFRACTIONOMETER

As the instrument is placed on the market, it is designed only for refraction at the center of the field. As modified by us, it can be used at any point in the horizontal meridian from the center of the field to an eccentricity of 60° temporally or nasally. Beyond this point the image reflected from the fundus is too dim to be distinguished. The instrument as modified for our purpose is shown in Fig. 1.

As seen in Fig. 1, the instrument is mounted on a carriage provided with ball and socket casters and made to pivot about a fixed point. The circle described by the carriage is graduated in degrees. The position of the carriage is read by means of a pointer which is located at the back of the carriage in the vertical plane passing through the principal axis of the optical system of the instrument. This pointer

passes over the graduated scale. In order that the eye may be located with precision vertically above the fixed point and thus be at the center of rotation of the instrument, a small disk is mounted at the point of intersection of the perpendicular erected at the fixed point with the principal axis of the optical system. When the eye is in position the front of the closed lid is in contact with this disk. To maintain the eye in this position, the observer is required to bite a mouthboard in which the impression of his teeth has previously been made and hardened in wax. The disk is then turned out of the way.

In order to facilitate the location of the eye in the desired position, the mouthboard is mounted on a rest provided with up and down, back and forth, and right and left adjustments. The adjustments are made by coarse-threaded screws. The screws for the right and left and the back and forth adjustments are driven by small wheels; the screw for the vertical adjustment is operated by a large milled head.

Method of controlling fixation and accommodation. It is supposed to be possible to use the instrument both with and without a cycloplegic. Both of these conditions have been used in this work. The directions given in the descriptive circular referred to above for the refraction of the central field without a cycloplegic are as follows: The patient is directed to look into the instrument with the eye under examination and to fixate the bright clear spot seen in the center of the field. This is for the control of fixation. For the control of accommodation the test target, which becomes clearly visible when it is located at the patient's far point, is always moved from a position beyond the far point towards the far point. The intention of this is to cause the patient to relax his accommodation in the interest of clear seeing.

We have not found this method to be entirely satisfactory. More reproducible results were obtained and results that check better with the subjective methods of refraction, by enlisting the aid of the eye not under examination; that is, by the use of both eyes, better control could be exercised over both fixation and accommodation. The procedure is as follows. When central refraction is to be determined, the eye not under examination is required to look at the smallest detail it can discriminate on a test chart at a distance of 6 meters. If it is hyperopic or emmetropic, it is fogged to 0.50 diopter of myopia to induce relaxation of accommodation. If it is myopic, it is already fogged for distance. In that case a larger detail to be discriminated has to be selected on the test chart. To guarantee that the eye not under examination is taking the same fixation as the eye under examination when it is looking at the bright dot at the center of the test field, as is required in the use of the instrument, the test chart is so located that the bright spot is seen at the center of the detail which serves for the fixation of the eye not under examination. This satisfies the two essential conditions; namely, that the two eyes are fixating in proper coördination and that the eye under examination is fixating the bright dot at the center of the test field. In refracting for the peripheral field the eye under examination can not, of course, be caused to look into the instrument. In this case both eyes fixate the distant chart whenever possible. This was possible only from 25° to 60°. That is, for the range 0° to 25°, when the temporal field was being refracted, the instrument was interposed between the test chart and the eye under examination. The fixation then had to be maintained by the

other eye alone. For the range 0° to 25°, when the nasal field was being refracted, the instrument was interposed between both eyes and the test chart. In this case the effect of distance was given to the eye not under examination by mounting close in front of it a small test letter placed at, nearer or beyond the principal focus of a double convex lens, thus causing the rays of light from the lens to enter the eye parallel, diverging or converging as desired. Coördination of fixation for the zero position of the instrument was secured by locating the object and the bright spot in the instrument in such relation that the test objects presented to the two eyes were seen as combined in the median plane. When a cycloplegic was used, the procedure was the same, with the exception that the fixating eye was not fogged.

With the eyes in position and the fixation held as described above, the refraction of the eye was determined at the center and in the nasal and temporal quadrants of the horizontal meridian at intervals of 5° and 10° out to 60°. At each angle of incidence tested, the refraction for both the horizontal and the vertical planes was recorded, *i.e.* the refraction in the plane of the incidence of the light and in the plane at right angles to it. The error of the determination was approximately 0.25 diopter in the central field and the mid-periphery, and approximately 0.37–0.50 diopter in the far periphery where the reflected images, owing to the high degrees of astigmatism present, were indistinct.

Results

A consideration of the effect of obliqueness of incidence on the refracting powers of a lens would lead one to expect a condition of myopia in the plane corresponding to the plane of incidence in case of an emmetropic eye with its refracting system symmetrically disposed in relation to the line to sight; that is, the effect of the oblique incidence in this plane is that of a strong plus cylinder. What the condition would be in the plane at right angles to the plane of incidence would depend upon two factors: (a) the effect of the obliqueness of incidence upon the focus in this plane, which, as already stated, is equivalent to that of the addition of a weak plus sphere; and (b) the shape of the eyeball. In proportion as the eye is spherical in shape, the retina from center to periphery is brought closer to the nodal point of the refracting system. In an emmetropic eye, therefore, with a symmetrical refracting system, one would expect a condition of hyperopia in this plane beginning at a point comparatively near the center of the field. At some point in the periphery in this type of eye a condition of mixed astigmatism might be expected. A mixed astigmatism might be expected also in case of small refractive errors at the center, hyperopic or myopic. In case of a sufficient amount of myopia at the center, a condition of compound myopia might be found in the periphery and similarly a condition of compound hyperopia in the periphery where a strong hyperopia is present in the central field. Obviously it is also possible that the shape of the curve of the eyeball from the posterior pole to-

wards the equator may be such as to cause either a compound myopia or a compound hyperopia even when there is only a slight error of refraction at the center. In this connection it should be remembered that in case of such a strong refractive system as is found in the eye, a very small difference in distance of the retina from the nodal point is sufficient to cause several diopters difference of myopia or hyperopia as the case may be.

Twenty-one eyes were examined in all, 15 without and 6 with a cycloplegic. The amount of refractive error at the center varied among these eyes from 0.25 diopter of astigmatism to several diopters of defect. In general, the effect of oblique incidence was an astigmatism which was variously modified in the eyes examined by the distances of the percipient elements from the nodal point of the refractive system. Classified as to peripheral refraction, 3 types of eye were found. In one type, A, the eye becomes more myopic in the horizontal plane as the periphery of the retina is approached, and more hyperopic in the vertical plane. That is, the curve representing the refractive errors for the horizontal plane tends downward as the periphery is approached and the curve for the vertical plane tends upward. In all the cases but one in which there was a high myopic astigmatism (central refraction), this resulted in a mixed astigmatism in the peripheral field, the interval of Sturm varying over a considerable range of diopters. In a second type, B, the eye becomes less myopic in the horizontal plane as the periphery of the field is approached and more hyperopic in the vertical plane; that is, the trend of the curve for both planes is upward. In the cases studied this resulted in a condition of compound hyperopia in the peripheral field for eyes that were hyperopic, emmetropic or moderately myopic in the central field. If strong myopia is present at the center it is obviously possible that a compound myopia might result over a greater part of the peripheral field. In a third type, C, the condition was asymmetrical, *i.e.* a considerable difference was found in the nasal and temporal halves of the field.

The results are shown in Figs. 2–6. In the charts shown in these figures the results for the horizontal plane, the plane of incidence of the light, are plotted as a solid line; for the vertical plane, the plane at right angles to the plane of incidence of the light, as a broken line. Degrees of eccentricity are plotted along the horizontal coördinate and diopters of refractive defect along the vertical coördinate. The condition of no refractive defect is represented by a horizontal line drawn through the center of the chart. Diopters of hyperopia are plotted above this line along the vertical coördinate; diopters of myopia below it. The diopters of hyperopia and myopia are expressed in terms of the correction needed, + and — respectively. The

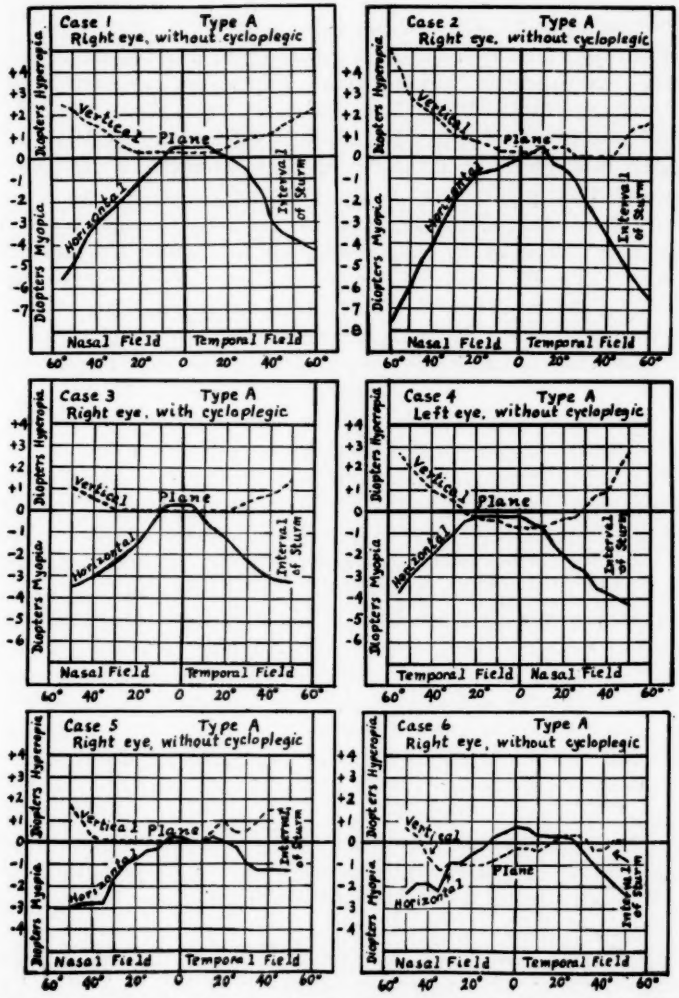

FIG. 2. REFRACTION OF PERIPHERAL FIELDS OF VISION FOR EYES OF TYPE A THAT HAVE ONLY A SLIGHT ERROR OF REFRACTION AT THE CENTER

value of the interval of Sturm can be read along the vertical coördinate at corresponding points between the solid and broken lines.

From the statement of principles already given, it is obvious that the shape of the curve drawn as a broken line is determined dominantly by the shape of the eyeball or by the distance of the percipient elements from the nodal point of the refractive system. That is, this curve represents the refraction in the vertical plane at the various points examined and the refraction in this plane is not strongly affected by the angle at which the examination is made. The shape of the curve drawn as a solid line, however, is strongly affected by the angle at which the examination is made. This curve represents the refraction in the horizontal plane, the plane of oblique incidence; and the effect is, as already stated, to bring the focus progressively nearer to the nodal point of the refracting system as the angle at which the examination is made is increased. The former curve thus informs us, roughly at least, as to the symmetry of conformation of the retina; and the latter, together with its relation to the former, gives us information as to the symmetry of action of the refracting system.

In Fig. 2 are shown results for 6 eyes of the class we have called Type A. These eyes all have only a slight refractive error at the center. It will be noted that in general the defect in the vertical plane, the amount of which is shown by the displacement of the broken line from the line of reference or zero line, is much smaller than the defect in the horizontal plane, shown by the displacement of the solid line. That is, the defect due to the obliqueness of incidence of the light on the refractive system is greater than the defect due to the progressive approach of the retina to the nodal point as the distance of the point under examination from the center of the field becomes greater. In short, in most cases much the greater part of the interval of Sturm is due to the astigmatism of oblique incidence. Some asymmetry will be noticed also in Case 6 but not enough to warrant placing it in the class which we have called Type C.

In Fig. 3 are shown the results for 6 eyes also of the class we have called Type A. These eyes have, however, a much more pronounced error of refraction at the center. The following points may be noted in connection with the results for this chart: (a) the curves are less regular; (b) the temporal and nasal halves of the curves are less symmetrical—Case 8, for example, shows so much asymmetry as to deserve, perhaps, to be classed as Type C; and (c) the point at which the mixed astigmatism begins is farther from the center of the field than is the case in Fig. 2—that is, a smaller proportion of the field carries a mixed astigmatism and a greater proportion a compound myopic or a compound hyperopic astigmatism. In Case 10, a

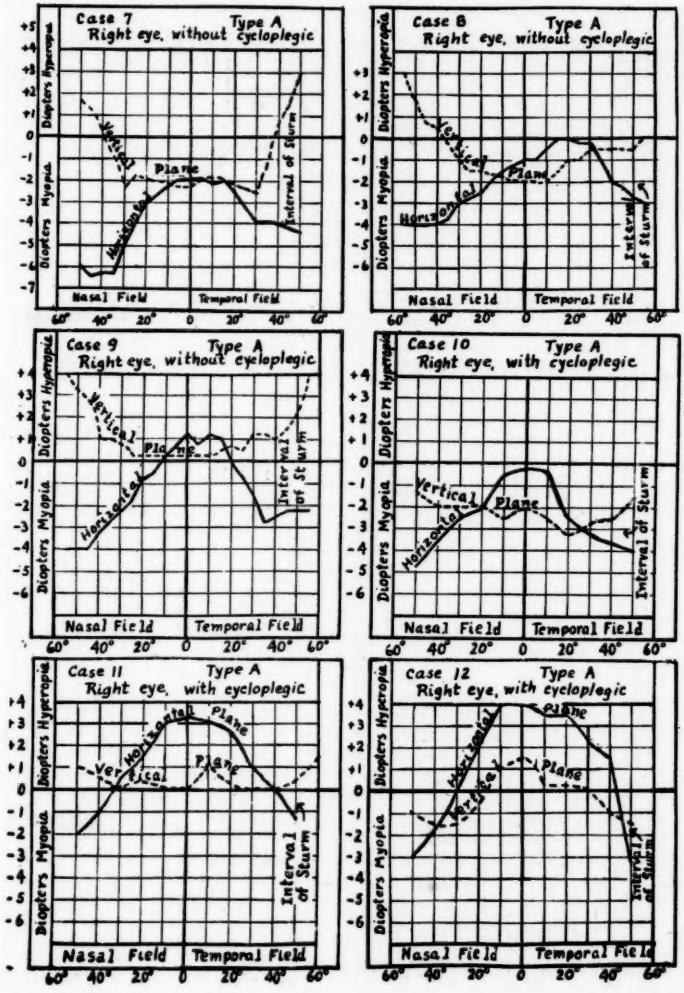

FIG. 3. REFRACTION OF PERIPHERAL FIELDS OF VISION FOR EYES OF TYPE A THAT HAVE A PRONOUNCED ERROR OF REFRACTION AT THE CENTER

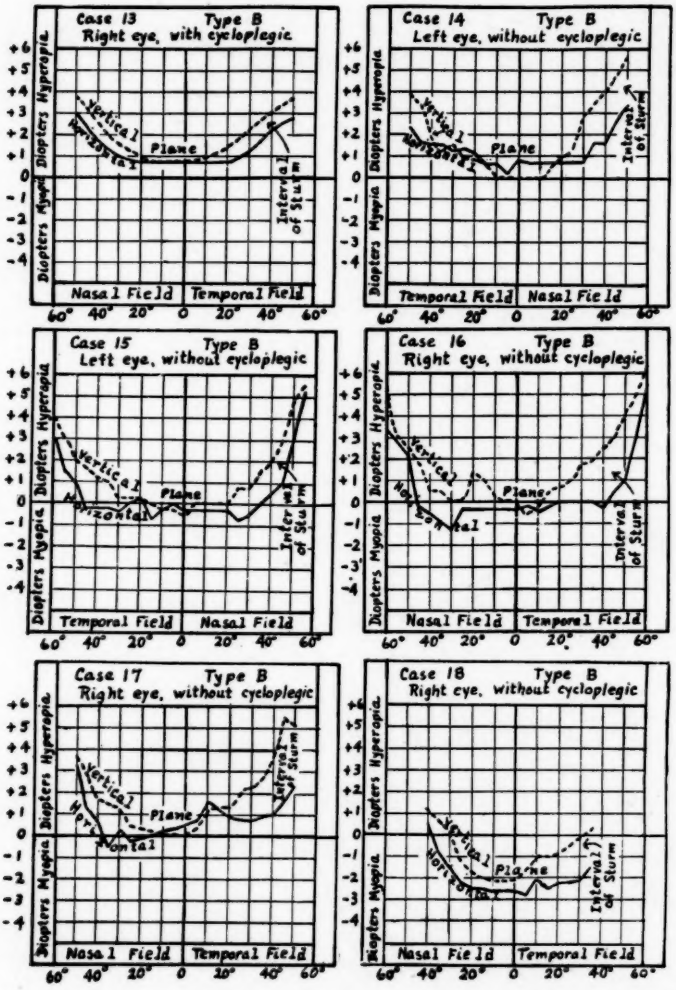

FIG. 4. REFRACTION OF PERIPHERAL FIELDS OF VISION FOR EYES OF TYPE B
Five have only a slight error of refraction at the center, one has a pronounced error.

compound myopic astigmatism with the greater myopia in the vertical meridian, the curves for the two meridians cross at approximately 20° to 25° from the center of the field; *i.e.* beyond these points the eye becomes more myopic in the horizontal plane and less myopic in the vertical plane, but it retains a compound myopia as far out as the examination was made. The astigmatism changed, however, from compound myopia with the rule to compound myopia against the rule.

In Fig. 4 are shown the results for 6 eyes which belong to the class we have called Type B. The characteristic defects in the peripheral field for these eyes are compound hyperopia and compound myopia. Four of the

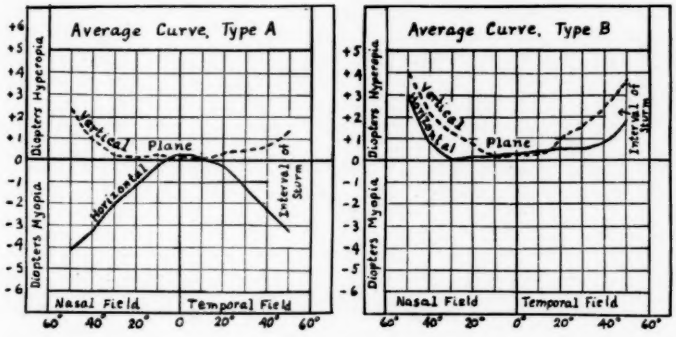

FIG. 5. COMPARISON OF THE RESULTS OF THE EYES OF TYPES A AND B

The chart at the left shows the average refraction of the eye for the peripheral field for 6 eyes of Type A having only a slight error of refraction at the center; that at the right, the average refraction for the peripheral field for 5 eyes of Type B having only a slight error of refraction at the center.

eyes have a slight refractive error at the center of the field and two a more pronounced error. As is shown in the chart, a striking feature of these cases is the smallness of the interval of Sturm in the peripheral field. In Type A the general trend of the curve for the vertical plane from center towards periphery is upward and of the curve for the horizontal plane downward. This gives a progressively increasing value to the interval of Sturm from center towards the periphery. In the cases of Type B, however, the trend of both curves from center towards the periphery is upward, the trend for the curve for the horizontal plane being less sharply upward. There is thus an interval of Sturm, but it is of smaller value than for the cases of Type A. It would appear that the astigmatism of incidence does not play proportionately so strong a rôle in the eyes of Type B as it does in the eyes of Type A.

In the left half of Fig. 5 are given curves showing the average of the results in the nasal and temporal halves of the field for 6 cases of the class we have called Type A. We have selected for this average only the cases which have a slight defect in refraction in the central field. This chart shows a slight asymmetry; *i.e.* in the nasal half of the field there is a greater amount of myopia in the horizontal plane and a greater amount of hyperopia in the vertical plane than there is in the temporal half of the field. In the right half of Fig. 5 are given average curves for the nasal and temporal halves of the fields for 5 of the cases we have called Type B. We have selected for the average curve here, also, the cases which have only a slight

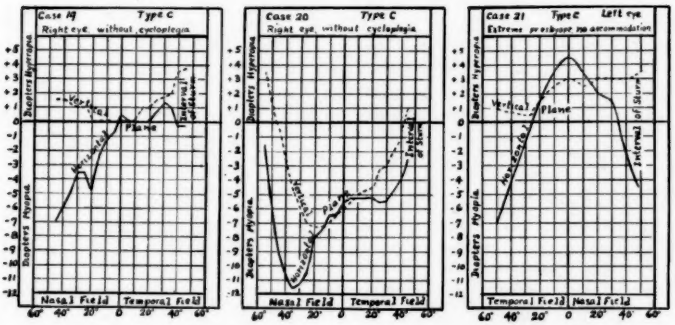

FIG. 6. REFRACTION OF THE PERIPHERAL FIELDS OF VISION FOR EYES OF TYPE C
These three eyes show marked asymmetry in the refraction of the nasal and temporal halves of the field.

defect of refraction in the central field. The average curves for the two halves of the field show less asymmetry, perhaps, than the curves in the left half of Fig. 5. Here too a greater refractive defect is shown in the nasal than in the temporal half of the field.

In Fig. 6 are shown the results for 3 eyes of the class we have called Type C. These cases, which show a pronounced difference in the two halves of the field, suggest (a) that the lens may not be symmetrically positioned with reference to the anterior-posterior plane of the eye; (b) that there may be asymmetry in the shape of the nasal and temporal halves of the eyeball in the meridian examined or a difference in the distance of the percipient elements from the nodal point of the refracting system in the two halves of this meridian from any cause whatsoever; or (c) that there is a combination of these two conditions. In the three cases shown in Fig. 6 a pronounced asymmetry was found in the refractive condition of the

temporal and nasal halves of the field and in one of these, Case 21, the asymmetry may sustain a causal relation to the ocular deviation which was found to be present. In the other two cases the character of the asymmetry is not such as to lead one to suspect any considerable effect on the muscle balance of the two eyes.

An inspection of Case 19 of this chart shows little asymmetry for refraction in the vertical plane; that is, the two halves of the curve for this plane (the broken line) show little difference in shape. For the refraction in the horizontal plane there is, however, a great deal of asymmetry. Largely because of the asymmetry in refraction in the horizontal plane there is also a pronounced asymmetry in the interval of Sturm in the two halves of the field. Under test this case showed practically no muscle imbalance.

An inspection of the chart shows no reason for expecting a muscle imbalance, so far as the refractive situation, central and peripheral, is concerned. That is, the eye is nearly emmetropic at the center of the field and no deviation in either direction could be expected to improve the refractive condition. The curves for this eye would seem to indicate a fairly symmetrical eyeball in the meridian under examination but an asymmetrical refracting system. The general shape of the curve for the refraction in the horizontal plane is such as might be expected for a lens rotated or tilted towards the temple. That is, the focus for objects in the nasal field is shorter than for objects in the temporal field as would be the case if the angle of incidence of light on the lens from objects in the nasal field was greater than from objects in the temporal field.

Case 20, Fig. 6, shows a pronounced asymmetry of the nasal and temporal halves of the field, but it appears to be an asymmetry due to a very considerable extent to the elongation of the eyeball in high myopia. An inspection of the curve representing the refraction in the vertical plane, the curve the shape of which is dominantly influenced by the shape of the eyeball, shows the maximum of myopia at about 15° to the nasal side of the center of the field or at a point on the retina about 15° to the temporal side of the fovea. The diopter value of the myopia here is about 7.25, at the center of the field it is 6. From the point of maximum value the curve rises more sharply towards the nasal than towards the temporal side of the field. However, at corresponding points in the two fields the myopia is greater in the nasal than in the temporal field. It is interesting to compare the shape of this curve with the shape of the eyeball in high myopia as is shown by the cross section of the eyeball given in Fig. 7. In the horizontal plane the value of the myopia at the center of the field was 5.5 diopters; at 35° it reached a maximum in the nasal field of 11.5 diopters; at 45° in the

nasal field it was 9 diopters and at 55°, 1.5 diopters. In the temporal field the value of the myopia at 30° was 5.5 diopters and at 45°, 2.5 diopters. A comparison of the shape of the two curves and of the interval of Sturm in the temporal and nasal fields indicates an asymmetry in the refracting system as well as in the shape of the eyeball, such as might be produced by a rotation or tilting of the lens towards the temple. The test both with the Maddox rod and with prisms shows 10 prism diopters of exophoria. When fixation was taken with the right eye alone, a deviation of 20° towards the temple was shown. Measurements of the cornea showed no asymmetry and the value of the curvature in both meridians fell within the normal range.

FIG. 7. CROSS-SECTION OF A MYOPIC EYE HAVING AN AXIAL LENGTH OF 28 MM. (From Fuchs: Textbook on Ophthalmology)

The color fields for red and blue, 1° stimuli, showed a contraction in the upper half. The field for 1° green was very small. The ocular deviation in this case was probably due to the high degree of myopia. Refractive asymmetry apparently had little or nothing to do with it. This may be inferred from the fact that when the myopia for the center of the field was corrected, the deviation practically disappeared and by the fact that a deviation of the eye either toward the temporal or the nasal side could not have been expected to render any very effective service in clearing up the defective imagery.

An inspection of Case 21, Fig. 6, left eye, shows asymmetry in the curves for both the vertical and horizontal planes. At the center of the field there was a high compound hyperopic astigmatism, 3 diopters in the vertical and 4.5 in the horizontal plane. In the nasal field in the vertical plane there was little change in the hyperopia as far from the center as 50°. In the temporal field it had decreased to 0.5 diopters at 25° and was 1 diopter at 50°. In the nasal field in the horizontal plane it decreased to zero at 35°; and at

50° there was 4.5 diopters of myopia. In the temporal field the hyperopia decreased to zero at 35°; and at 50° there was 7 diopters of myopia, just twice the amount present at the corresponding point in the nasal field. It is significant to note that the nearest approach to emmetropia in the eye came at 25° from the center in the nasal field. The defect here was 0.5 diopter of simple hyperopic astigmatism.

The vision in this eye was 20/200 and could not be improved substantially by correction. The patient stated that prior to 15 years ago there was, so far as he knew, no vision at all in this eye. An examination for central scotoma gave negative results. That is, while the test object was seen

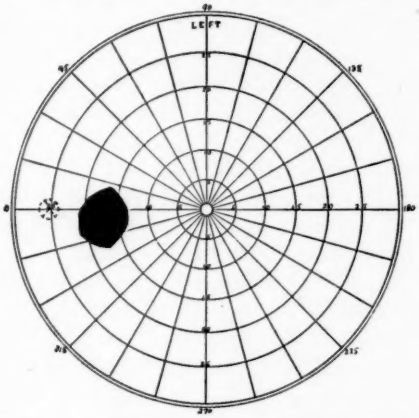

FIG. 8. BLIND-SPOT AND FIXATION-POINT FOR CASE 21

vaguely at the center it was seen equally clearly there and in the field surrounding the center for 15 degrees to 20 degrees in all directions. A test with the Haitz charts showed that when both eyes were open the vision in this eye was entirely suppressed. When the right eye was closed, however, the figure for the left eye in the chart was seen. When both eyes were open there was no observable deviation in either eye; but when the right eye was closed the left immediately turned inwards and took a fixation approximately 25° from the median plane. It will be remembered from Fig. 6 that the nearest approach to emmetropia in the left eye was at an eccentricity of 25° in the temporal field. When using this eye alone, the patient apparently turned the eye inwards about 25° in order that objects in front should be in the line of the least refractive error and the clearest image be formed on the retina.

In the course of the examination of the eye the blind spot was mapped on the tangent screen of the Ferree-Rand perimeter with a 0.5° white stimulus-object on a black background. With the eye turned towards the center of the arc of the perimeter, the fixation-object could not be seen with sufficient clearness to give a satisfactory control of fixation. In order to secure a correct fixation, the left eye was lined up by means of the telescope inserted in the hollow axle of the perimeter. A large fixation-cross, 3 cm. in diam., was moved temporally along the horizontal meridian to the point at which it could be seen most clearly. The highest visibility was attained at a point approximately 25° from the center of the arc. This, it will be remembered, was the point in the field at which there was the smallest error refraction, 0.5 diopter simple hyperopic astigmatism. With the fixation-object at this point a fairly satisfactory control of fixation was secured and a map of the blind-spot was made. The location of the fixation-object and the map of the blind-spot are shown in Fig. 8.

A phenomenon of very great theoretical and practical interest observed during the course of the work may be noted here. During the early part of the work of the examination at the different point in the peripheral field was made by taking an eccentric fixation, the instrument remaining stationary. One of the reasons for abandoning this method of working was the fact that when the fixation was held for any appreciable length of time, the eyes of the class we have called Type A developed an increased myopia in the horizontal meridian with a corresponding decrease in the hyperopia for the vertical meridian. With prolonged fixation this change in some cases amounted to as much as 2.5 diopters. A similar tendency in the same direction was shown also by eyes of the class we have called Type B. Further, when the method was changed and a far fixation was taken in the median plane—the examination at the peripheral points in the field being made by rotating the apparatus about the fixed point described in the method of working—the phenomenon was still present in some eyes but to a lesser degree. There seem to be two possibilities of explaining the phenomenon: (a) a change in the convexity of the lens, and (b) the possibility that as the fixation is prolonged the muscles of the eye gradually produce an elongation of the eyeball.[3] The first of these explanations does not seem plausible because the rendering of the eye myopic by a change in

[3] It is assumed here that the eyes are under considerable pressure from the external muscles in a prolonged eccentric fixation, the amount depending upon the degree of eccentricity. It is also assumed that they are under some pressure in taking and holding far fixation in the median plane; that is, when relaxed and freed from all incentives to adjustment for clear seeing, as is the case with closed lids and in the dark, the eyes take a more converging position.

the focus of the lens would operate against the clear seeing of the fixation-object. This would be in opposition to one of the eye's strongest reflex incentives; namely, the adjustment of its refracting mechanism to produce the clearest possible vision; while the gradual elongation of the eyeball because of the pressure exerted by the external muscles in maintaining fixation, falls entirely outside the reflex and is unavoidable.

To test the point, the examination was made under as profound a cycloplegic as could be obtained with atropine. The performance of the Zeiss refractionometer is, it may be noted, not as satisfactory with as without a cycloplegic. With wide expansion of the pupil, produced by the cycloplegic, the test images that are formed by the refracting system of the eye are not nearly so clear and distinct as they are with smaller pupils. This affords another interesting example of the poor and unnatural performance of the refraction system of the eye with an expanded pupil. It was found possible, however, to make the examination under cycloplegic with a fair degree of satisfaction. The phenomenon was still present, but in a diminished amount.

In connection with the phenomenon two points of interest may be noted: (a) there is a bearing on the possibility of a permanent elongation of the eyeball by the pressure of the external muscles, one of the theories of the cause of myopia; and (b) the phenomenon strongly suggests that a temporary elongation may be produced in some eyes, presumably due to the action of the external muscles. This is of interest in connection with theories of accommodation and with the ability of certain aphacic eyes to see near objects with the same correction that is used for seeing distant objects.

Summary and Conclusions

(1) The refraction of the eye in the temporal and nasal peripheral field as far out as 60° has been studied by means of a Zeiss refractionometer modified to suit the purpose. Twenty-one eyes have been examined. Those selected included eyes with a slight central error of refraction and eyes with a pronounced central error of refraction.

(2) The eyes examined fall into three classes. In Type A, the eye becomes more myopic in the horizontal plane as the periphery of the field is approached and more hyperopic in the vertical plane. In all eyes examined but one (in which there was a high myopic astigmatism in the central field), this resulted in a mixed astigmatism in the peripheral field, the interval of Sturm varying over a considerable range of diopters. In Type B, the eye becomes less myopic in the horizontal plane and more hyperopic in the vertical plane. This results in a condition of compound hyperopic astigmatism in the peripheral field for eyes that are hyperopic, emmetropic

and moderately myopic in the central field. If there is strong myopia at the center of the field, it is obviously possible that a compound myopic astigmatism might result over the greater part of the peripheral field, weakening in amount as the periphery is approached. In Type C, the refractive condition is asymmetrical for the nasal and temporal halves of the field. The general trend of this asymmetry, as the periphery of the field is approached, is towards greater myopia and less hyperopia for one half of the field than for the other half.

(3) As fixation was prolonged, a tendency toward myopia was found; that is, if hyperopic, the eye tended to become less hyperopic; if emmetropic, it tended to become myopic; and if myopic, more myopic. This change was found to take place in all the eyes examined when an eccentric fixation was taken; also for some eyes when a far fixation was taken in the median plane. Further, the change was found to take place whether the examination was made with or without a cycloplegic. Two points of interest may be noted in connection with this phenomenon: (a) there is a bearing on the possibility that a permanent elongation of the eyeball may be produced by the pressure of the muscles, one of the theories of the cause of myopia; and (b) the phenomenon suggests that a temporary elongation may be produced in some eyes, presumably due to the action of the external muscles. This is of interest in connection with theories of accommodation and with the ability of some aphacic eyes to see near objects with the same correction that is used for seeing distant objects.

(4) The following additional points may be noted: (a) the important relation which the refractive conditions in the peripheral field sustain to acuity in the peripheral field, to achromatic and chromatic sensitivity in peripheral vision and the limits of the form and color fields, and to the anomalies and irregularities in peripheral space perception. The conditions for the formation of an image are so bad for objects in the peripheral field that one can only marvel that peripheral vision is as good as it is, and that the peripheral portions of the retina should have attained as high a development as they have.

(b) The possible bearing of asymmetrical refraction in the peripheral field on the explanation of cases in which central vision can not be substantially improved by correction in eyes which show no central scotoma.

(c) The rôle which asymmetrical refraction may play in cases of ocular deviation and the bearing that the demonstration of such a condition may have on the treatment of the case.

(d) The possibility of determining roughly the conformation of the retina and the shape of the posterior half of the eyeball by refracting the eye

for the peripheral field; also of determining within rather wide limits in cases of refractive asymmetry whether the defect is in the refracting system, or in the conformation of the retina, or in both.

(e) The possibility and comparative practicability of using the Zeiss refractionometer, modified as described in this paper, for studying the refractive conditions in the peripheral field and in testing for refractive asymmetry. To give the instrument maximum serviceability for this purpose, however, a further modification should be devised to make the examination possible and feasible for the upper and lower as well as for the temporal and nasal halves of the field. Cases which might be tested to advantage for refractive asymmetry as clinic procedure are those in which a pronounced ocular deviation is combined with low central vision and there is no detectable scotoma.

It is stated above that one marvels that vision in the peripheral portions of the field is as good as it proves to be. In fact when one considers the type of image that is formed in case of an axial astigmatism, an astigmatism of incidence or a lens out of focus, in comparison with the power of vision that is actually present under these conditions, one is almost inclined to concede a supplementary resolving power to the rods and cones themselves. Some of the additional phenomena that seem to need explanation are: (1) the power of vision in general in ametropic eyes; (2) the power of vision in the peripheral field; (3) the power of seeing objects nearer and farther than the fixation-point; (4) the contention by Ames and Gliddon that the actual accommodation distance in viewing objects does not coincide with the apparent accommodation distance;[4] (5) the power which some aphacic eyes have of seeing objects at all distances with the same pair of glasses; (6) the effect of increase of intensity of illumination on acuity and the much greater effect on ametropic than on emmetropic eyes; and (7) the phenomenon of relative accommodation, on the power of separating accommodation from convergence without impairing vision, and the increase in this power with increase of intensity of illumination.

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[4] A. Ames, Jr., and G. H. Gliddon, Ocular measurements, *Trans. Amer. Med. Assoc., Section on Ophth.*, 1928, 102-169.

PITCH DISCRIMINATION NEAR THE UPPER AND LOWER THRESHOLDS OF AUDITION

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This paper reports a study of pitch discrimination near the upper and lower thresholds of audition. Most previous work with extremely high and low tones has been concerned with determination of tonal stimulus-thresholds. This study was made in an endeavor to find out if there is a range of very low pitches which can be heard as tones but which cannot be discriminated as higher or lower than each other, and also if there is a like range near the upper threshold. It was undertaken as a result of a suggestion coming from Hardesty's work with a model designed to illustrate the probable action of the tectorial membrane.[1] He found that while different lengths of the elk-hide in his model vibrated for frequencies from *a* to *f'*, for frequencies below *a* the whole membrane vibrated. If the tectorial membrane in the ear should be the primary vibrating structure of the ear, and if it behaves in a fashion similar to Hardesty's model, then it might reasonably be expected that there would be a range at the lower end of the pitch series in which all frequencies would produce the sensation of the same pitch.

Values for the lower threshold tabulated by Titchener from nineteen references up to 1905 range from 7d.v. to "between 16 and 48."[2] Apparatus used includes large pipes, loaded reeds, rotating spokes, large tuning forks, and the Appunn lamella. Many authors mention that it is easy to mistake the first overtone of one of these sound generators for the fundamental. Dunlap and Wells outline the following method for avoiding this error in work with the lower threshold.[3] *O* is presented with a tone from a tuning fork set to 42 d.v.; if he hears this plainly as a tone he compares it with a tone from a 21 d.v. fork. If the tones from the second fork seem clearly lower than tones from the first, then lower pitches always an octave apart are to be compared until *O* judges the tones from both forks to be not different. If the tones from the forks set at 21 and 42 d.v. are judged the same, higher pairs are to be compared until a point is reached at which tones from the

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[1] I. Hardesty, A model to illustrate the probable action of the tectorial membrane, *Amer. J. Anat.*, 18, 1915, 471-514.

[2] E. B. Titchener, *Experimental Psychology, Quantitative Experiments, Instructor's Manual*, 1905, 24-31.

[3] K. Dunlap and G. R. Wells, *Laboratory Exercises in Psychology*, Part I, 159-161.

two forks are judged different. Dunlap and Wells suggest that when tones from forks set an octave apart seem the same in pitch "the tone of the slower fork is the second partial of its fundamental note; the partial being probably an artifactual one produced in the middle ear."[4]

Vance reports that 5 trained *O*s after practice could hear tones from 12 and 13 d.v. forks.[5] It is still a question whether these *O*s actually heard tones of 12 d.v. or an artifactual overtone. Fletcher emphasizes that intensity is an important factor in the determination of the lower threshold and that most previous work is inconclusive because this factor has been neglected.[6] The upper and lower thresholds in the audiograms of Fletcher[7] and Wegel[8] are obtained by extending the curves for minimum audible pressure and maximum audible pressure. Wegel says, "As the frequency is still decreased (below 60 d.v.) to the point of intersection of the feeling and hearing lines it is difficult to distinguish between the sense of hearing and that of feeling. . . . Below the point of intersection it is easier to feel than to hear the air vibrations. . . . At the lower limit the excursions of the diaphragm and ossicles are probably so large that the nerves feeding these movable parts are stimulated."[9] The resulting flutter sensation constitutes a practically inescapable secondary criterion in tests with low tones.

Stückert,[10] Gradenigo,[11] Vance,[12] and Kuehl[13] have all found difference sensitivity finest in the middle range and increasingly grosser towards the extremes. Maltzew found interval judgment much more faulty at very high and low ranges than at the central.[14] Achmatov found no differential adaptation effect for three widely scattered pitches.[15]

Experiment I

Apparatus and procedure. For the production of low tones as pure as are available at present three large tuning forks were used in Experiment I. The weights on the largest fork in Experiment I could be set so that it would give from 16 to 24 d.v., the middle sized fork could be set from 24 to 38, and the smallest from 36 to

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[4] *Ibid.*, 161.

[5] T. F. Vance, The lower limit of tonality, *Psychol. Monog.*, 16, 1914, (no. 69), 104-114.

[6] H. Fletcher, Physical measurements of audition and their bearing on the theory of hearing, *J. Franklin Inst.*, 196, 1923, 289-326.

[7] *Ibid.*

[8] E. P. Fowler and R. L. Wegel, Audiometric methods and their applications, *Trans. Amer. Laryng. Rhinol. and Otol. Soc.*, 1922, 98-131; R. L. Wegel, The physical examination of hearing and binaural aids for the deaf, *Proc. Nat. Acad. Sci.*, 8, 1922, 155-160.

[9] Wegel, *op. cit.*, 157.

[10] N. Stückert, Über die Unterschiedsempfindlichkeit für Tonhöhen in verschiedenen Tonregionen, *Zsch. f. Sinnesphysiol.*, 42, 1908, 392-408.

[11] G. Gradenigo, Über die Empfindlichkeit des Gehörorgans für die verscheidenen Regionen des Tonleiters, *Arch. f. Ohrenhlk.*, 96, 1914, 103-114.

[12] T. F. Vance, Variation in pitch discrimination within the tonal range, *Psychol. Monog.*, 16, 1914, (no. 69), 115-149.

[13] B. F. Kuehl, Measurements of auditory acuity with the Iowa pitch range audiometer, *Psychol. Monog.*, 31, 1922, (no. 140), 83-97.

[14] C. von Maltzew, Das Erkennen sukzessiv gegebener musikalischer Intervalle in den äusseren Tonregionen, *Zsch. f. Psychol.*, 64, 1913, 161-257.

[15] A. S. Achmatov, Über die akustische Adaptation, *J. f. Psychol. u. Neurol.*, 31, 1925, 428-442.

55. They were clamped to a heavy oak table at fixed positions 2 in. apart. The table was placed near a corner of the room.

A black cloth draped over a standard concealed the apparatus from the *O*s. A conical megaphone whose diameter at the large end was approximately 13 in., and whose long axis was a little less than 28 in. was hung so that the central part of the large end was about 1½ in. from the upper halves of the two forks in use, and the other end protruded through the curtain at the height of *O*'s ear. *O* sat on a stool the height of which could be adjusted so that he could comfortably hold his ear at the opening. The use of the megaphone enabled *O* to hear the tuning fork tones while at sufficient distance to obviate spatial cues from the positions of the forks.

The fundamental tone of the megaphone, obtained by blowing a jet of air across its mouth, was located at about *g*♯ by several *O*s comparing it with a tone obtained in the same manner from a Helmholtz resonator. This was much higher than the pitch of the highest fork tone used and hence, according to Miller,[16] this horn should not have distorted the tuning fork tones appreciably.

A set of wooden clips was used to set the forks in vibration. These were made from small wooden blocks by chiseling out in them shallow rectangular holes wide enough to hold the two prongs of a fork when these were pulled together. When *E* drew off the clip the forks would vibrate immediately. Several graduate students assisted in selecting the clips to be used for various frequencies. They sat with their ears to the end of the megaphone while the experimenter tried various clips for the octave pairs of forks, and reported when tones from the two forks seemed most alike in intensity. It was not possible to get equal intensities for all pairs of forks as the *O*s disagreed as to intensity differences. The clips used gave a tone of moderate to weak intensity; louder tones would have been desirable but could not be obtained without increasing a great deal the disturbance from forced vibration in the table and floor. Felt pads were placed under the table legs to minimize this forced vibration, and a heavy curtain several times folded was placed unevenly on the floor below the table to prevent the air space below the table from acting as a resonance chamber for any one frequency. *E* kept his recording material in clamps fastened to the wall so that no vibratory object of any kind was present on the table.

Until oscillographic or galvanometric records of these fork tones are made and analyzed it will not be known what tones they produced besides the fundamental. The 6.27 or another overtone was heard as a little, rather faint 'singing' tone in the middle range by some of the *O*s[17]; they were told to disregard this by *E*, who compromised his honesty to the extent of telling them that this tone would not help them one way or another in their judgments. It did not occur to him till later that this might have acted as a negative suggestion. Most of the *O*s claimed to have disregarded this overtone, though there is no telling how much it may have influenced them unconsciously. Whether the second harmonic partial was objectively present is another question. If present in one fork at one frequency it probably was present at all times with all frequencies, since the material of the fork and the manner of generating the tone was the same for all. However, the relative posi-

[16] D. C. Miller, *The Science of Musical Sounds*, 1916, 156-162, 186-188.
[17] S. H. Anderson, Overtones of large tuning forks, *Phys. Rev.*, 21, 1923, 692-694.

tion of the weights on the forks varied. On the largest fork the weights were close to the top of the prongs for 16 d.v. and rather close to the center for 24.

This technique for producing low tones, while adequate for a preliminary study such as this, would be imperfect and unsatisfactory for final results, but perhaps no more so than other means available at present. If the forks had been set in vibration by a narrower clip giving the prongs a greater original displacement the forced vibration in the floor would have been increased. Oscillatory electric apparatus if used with a telephone receiver diaphragm would give a tone containing upper partials due to the diaphragm as well as the cue from the flutter sensation.

At the beginning of the experiment *O* was seated with his ear to the small opening of the megaphone. He was told that he would hear a sound which he was to describe. Then one of the tuning forks was sounded. Most of the *O*s described the sound as a rumble, some as a flutter, one as a "moan," another as "like a train coming in." After tones of several different vibration rates had been sounded the *O*s were able to accept the sounds as tones having a certain amount of pitch. *O* was then told that he would be given a series of pairs of tones and that he was to judge the second tone of each pair as higher, lower, or the same in pitch as the first. Two forks were then set to frequencies an octave apart. Each series was made up of 12 pairs: 3 in which tones from the higher fork preceded those from the lower, 3 in which the lower preceded the higher, 3 in which the higher fork was sounded twice, and 3 in which the lower fork was sounded twice. *E* gave a ready signal, sounded one fork for 1½-2 sec., damped it with the gloved hand, and after a pause of about 1 sec. sounded the same fork or the other fork for 1½-2 sec. The order of the pairs within the series of 12 experiments followed lists of orders made up by chance. Nine series were given: one each for the octave pairs 16-32, 17-34, 18-36, 19-18, 20-40, 21-42, 22-44, 23-46, and 24-48 d.v. The succession in which these series were given was from high to low (or from low to high) but shuffled, as for example: 16-32, 19-38, 17-34, 18-36, 21-42, 20-40, 24-48, 23-46, 22-44. It was thought that this shuffling might help prevent the carrying over of secondary criteria from high overtones, etc., from one octave pair to the next given pair.

There were 12 *O*s in this experiment, all of them advanced students at the Peabody Conservatory of Music (except *Q*, who had previously studied there). They all had pitch discrimination thresholds within 3 d.v. at 435 d.v. The experimental periods were rather irregularly spaced: at first most *O*s found this experiment difficult and fatiguing, so not more than two series were given a day. Later, as the *O*s became more adapted to the experiment, 4 or 5 series were given a day. The time which the *O*s could give to experimental work varied from day to day so that on some days several series could be given and on others none at all. Distracting factors were the moan of the wind about the corner of the building, noises from automobiles, trucks, and sometimes from airplanes. In cases where one or both tones in a pair were not clearly heard the pair was repeated later in the series, so that for each series 12 comparatively clear judgments were obtained.

The *O*s (*A, B, C, D, F, G, H, L, M, N, P* and *Q*) gave 12 judgments each on the 9 octave pairs of forks from 16-32 to 24-48 d.v. Nine of the *O*s (all but *C, F,* and *Q*) repeated the experiment. Thus a total of about 2268 judgments was received.

TABLE I

TOTAL NUMBER OF JUDGMENTS OF ALL *O*s ON EACH PAIR OF FORKS

		Sequence											
		HL			LH			HH			LL		
Forks	Section	L	H	S	L	H	S	L	H	S	L	H	S
16–32	1	18	13	5	9	22	5	6	6	24	6	10	20
	2	21	6	0	12	15	0	1	3	23	4	6	17
	T	39	19	5	21	37	5	7	9	47	10	16	37
17–34	1	14	13	9	9	17	10	2	8	26	9	7	20
	2	20	3	4	9	15	3	4	4	19	3	2	22
	T	34	16	13	18	32	13	6	12	45	12	9	42
18–36	1	8	19	9	16	14	6	4	5	27	6	10	20
	2	14	11	2	10	16	1	7	2	18	4	5	17
	T	22	30	11	26	30	7	11	7	45	10	15	37
19–38	1	14	19	3	19	12	5	4	10	22	4	12	20
	2	7	18	2	19	6	2	5	1	21	1	5	21
	T	21	37	5	38	18	7	9	11	43	5	17	41
20–40	1	10	22	4	24	9	3	7	4	25	7	8	21
	2	9	12	6	12	7	8	6	2	19	3	5	19
	T	19	34	10	36	16	11	13	6	44	10	13	40
21–42	1	10	18	8	21	7	8	9	5	22	8	9	19
	2	11	16	0	15	6	6	3	3	21	7	4	16
	T	21	34	8	36	13	14	12	8	43	15	13	35
22–42	1	10	20	6	16	11	7	7	6	22	1	14	20
	2	6	15	6	20	4	3	3	3	21	5	7	15
	T	16	35	12	36	15	10	10	9	43	6	21	35
23–46	1	14	18	4	16	13	7	7	5	24	4	10	22
	2	8	17	2	19	6	2	3	3	21	4	3	20
	T	22	35	6	35	19	9	10	8	45	8	13	42
24–48	1	15	20	1	18	12	6	6	4	26	4	4	28
	2	6	18	3	18	8	1	3	4	20	7	0	20
	T	21	38	4	36	20	7	9	8	46	11	4	48

Results. The total number of judgments of higher, lower, and same for each sequence on each fork pair is given in Table I. In this table judgments for each sequence for each fork pair total 36 for the first section and 27 for the second, since while 12 *O*s completed the first section only 9 repeated it. The total number of judgments on all fork pairs for every *O* are given in Table II.

From these tables three conclusions may be drawn. First, the judgments on the HL (high-low) and LH (low-high) pairs are very erroneous. The judgments on the LL (low-low) and HH (high-high) are somewhat better but there is still a great deal of uncertainty. Secondly, there is a tendency for the same fork tone to be judged 'same' (S) more often in the second

TABLE II

TOTAL NUMBER OF JUDGMENTS ON ALL FORKS FOR EVERY *O*

		Sequence											
		HL			LH			HH			LL		
O	Section	L	H	S	L	H	S	L	H	S	L	H	S
A	1	7	12	8	13	8	6	3	6	18	4	11	12
	2	20	4	3	10	13	4	8	0	19	6	10	10
B	1	10	17	0	20	6	1	4	6	17	8	5	14
	2	8	18	1	19	7	1	0	1	26	1	1	25
C	1	9	15	3	6	13	8	1	3	23	1	6	20
D	1	6	15	6	19	4	4	4	3	20	3	5	19
	2	8	13	6	19	5	3	0	2	25	0	0	27
F	1	12	12	3	9	12	4	4	6	16	6	6	14
G	1	13	13	1	18	8	1	9	5	13	4	8	15
	2	9	18	0	18	5	4	2	1	24	4	1	22
H	1	12	9	6	10	14	3	4	3	20	4	9	14
	2	10	16	1	16	9	2	4	6	17	3	5	19
L	1	3	21	3	13	12	2	6	12	9	3	15	9
	2	10	16	1	16	10	1	1	5	21	3	10	14
M	1	18	6	3	6	13	8	12	2	13	13	7	7
	2	18	5	4	7	14	6	11	7	9	12	8	7
N	1	9	11	7	8	10	9	1	4	22	0	5	22
	2	9	12	6	11	8	8	2	1	24	1	0	26
P	1	10	15	2	17	7	3	4	2	21	3	2	22
	2	10	14	3	20	6	1	7	2	18	4	2	21
Q	1	4	16	7	9	10	8	0	1	26	2	5	20

section of the experiment than in the first. This tendency is more marked with the LL sequence: *B, N, D, H, L,* and *G* gave more judgments of 'same' on LL pairs in the second section than in the first, while *A, M,* and *P* gave about the same number in both. With the HH sequence *B, D, G,* and *L* gave more judgments of 'same' in the second section; *A* and *N* gave about the same number; and *H, M,* and *P* gave fewer. Thirdly, the HL and LH pairs on which the majority of judgments are correct are the two lowest pairs: 16-32 and 17-34; the other higher pairs received a majority of incorrect judgments. This is in direct contradiction to the natural expectation that the higher pairs would be the more correctly judged.

These results from *O*s with fine pitch discrimination at ordinary ranges are significant, because while Stumpf found some evidence that a tone could be confused with its octave, there is no reason for expecting a tone an

octave higher than another tone to be rather consistently reacted to as lower. It may be that intensity differences in the fork tones are somewhat to account for this and the other discrepancies, but if so, it is significant that the pitch discrimination of advanced music students should be so poor at this range as to allow an incorrect system of pitch judgments to be built up from the very moderate intensity differences present in this experiment. Our *O*s would never at ordinary pitch ranges judge a loud *c'* an octave higher than a softer *c*, or a faint *A* an octave higher than a louder *a*. Another possible explanation is that if from a 20 d.v. fork only an artifactual overtone of 40 d.v. is heard this overtone may sound lighter in quality or at least different from the objective vibrations of a 40 d.v. fork, which supposedly would also produce its own artifactual overtone of 80 d.v. If the 40 d.v. artifact sounds lighter than the objectively produced 40 d.v. tone it might come to be consistently regarded as higher. But the clips used were selected empirically to give approximately equal intensities, the *O*s were instructed to judge on the basis of pitch and all reported trying to do so, and while *O*s were only occasionally questioned as to intensity differences in the fork tones they sometimes reported the higher tone as louder than the lower, and sometimes the reverse. It is possible, though, that many of the correct judgments of 'same' were due not only to a feeling of sameness in pitch but also to a feeling of sameness in intensity and vibration as well.

*O*s were not questioned during the experiment as to whether they judged the tones with faster vibrations lower or higher unless they brought up the point themselves. Such questioning was avoided because it was not desirable to direct the attention to the vibration sensation as a criterion of pitch, but this may have been a useless precaution. *B* said she judged the tones vibrating faster as higher, those vibrating slower as lower. *Q* did the reverse. *M* on the 18-36 series and *N* on the 17-34 each reported that tones could seem to be the same pitch but "coming at a different rate;" *M* didn't see how this could be. *Q* reported much difficulty in judging the pitches of these tones and said he was forced to judge the "lighter quality higher." *H* reported of the 24-48, 21-42, 17-34, and 16-32 series that the higher tones (really lower for the first two pairs and higher for the last two) were harder to hear; maybe they had a low fundamental that he couldn't hear. Later he reported that in cases where one tone (23 d.v.) was judged a twelfth higher than the other (46 d.v.) that the interval really sounded like a fifth but that he felt that the fundamental was lacking in the second tone (23 d.v.).

The *O*s were fairly consistent among themselves in their reports of the

intervals between different tones. *A, B, C,* and *L* reported that tones from the lower pairs seemed a semitone or whole tone apart; *N* reported hearing thirds and *P* sevenths or ninths at these points. The middle pairs were called a semitone or whole tone apart by *B, C, L,* and *N,* and a third apart by *A, D, L, N,* and *P.* The higher pairs were judged as a fourth, fifth, or more apart by *A, C, D, G, L,* and *P.* These interval judgments were not required but the *O*s were usually asked at the end of each period if they had anything to say about the intervals between notes judged as different. Some of the *O*s expressed dislike for this experiment because it caused a strain on the attention and they felt so much at sea in their judgments.

TABLE III

JUDGMENTS ON TONES FROM PAIRS OF LOW TUNING FORKS HELD SUCCESSIVELY TO *O*'s EAR

		Sequence											
		HL			LH			HH			LL		
Forks	*O*												
16–32	*D*	L	L	L	H	H	H	S	S	S	S	S	S
	F	L	S	L	L	S	L	S	L	S	S	S	S
	G	S	L	L	L	H	H	S	S	H	L	S	L
	K	S	L	L	H	H	L	L	S	L	L	L	S
	N	S	S	S	S	S	H	S	S	S	S	S	S
19–38	*D*	L	L	L	H	H	H	S	S	S	S	S	S
	F	L	L	L	H	H	H	S	S	S	L	L	S
	G	L	L	L	H	L	S	S	L	H	H	S	S
	K	L	L	L	H	H	H	S	L	L	L	S	S
	N	S	S	L	H	H	S	S	S	S	H	S	H
21–42	*D*	L	L	L	S	S	S	S	S	S	S	S	S
	F	L	L	L	H	L	L	L	L	S	H	L	S
	G	S	H	L	H	L	H	H	H	S	S	H	S
	K	L	L	L	H	H	H	S	S	S	S	S	S
	N	L	L	L	H	H	H	S	L	L	S	S	S

At the end of this experiment 5 *O*s were put through a series with the 16-32, 19-38, and 21-42 octave pairs. In this case the forks were struck on a sandbag and held to *O*'s ear. The higher forks were held from 5-6 in. from the ear and the lower forks much closer to minimize intensity differences. This technique does not exclude cues or disturbances from the flutter sensation, which was quite noticeable, nor from the striking: the lower fork had to be struck more strongly than the others. Table III gives the judgments on these pairs. The total number of judgments is too few for any thorough comparison of this method with the other, but it will be noted that the frequency of correct judgments is much greater. These tones had a greater intensity than those in the main experiment and it is possible that these tones, being stronger would have stronger artifactual overtones: what might be heard from the 16 d.v. fork would be a 32 d.v. artifact, and from

a 32 d.v. fork the objective 32 d.v. tone and a 64 d.v. artifact; naturally the second would sound higher. There is also a possibility that tuning forks struck as hard as these would objectively produce the first overtone.

Summary and conclusions. To conclude, music students with pitch discrimination well above the average have great difficulty in comparing the pitches of octave pairs of tones from 16 to 48 d.v. produced in the manner described above, they cannot do so accurately, and are evidently often forced into the use of secondary criteria from intensity and vibratory sensations. If it should be true that discrimination is better for tones louder than those used in the main experiment, then our results point to a relation between intensity and discrimination at this pitch range, something for which there is absolutely no parallel at ordinary ranges for pitch differences so large and intensity differences so small.

Experiment II

Apparatus and procedure. A supplementary experiment on pitch discrimination at low ranges was carried out with notes from the Bourdon unit stop in a Moller pipe organ.[18] This unit consisted of stopped wooden pipes and gave comparatively pure tones, the purest available in the organ at as low a pitch range (about 32–44 d.v.).[19]

Eight *O*s took part in this experiment. Preliminary tests showed that all could correctly distinguish as higher or lower tones a half tone apart at ranges above 65 d.v. They were tested individually, sitting in the choir loft of the church with their backs to *E*, who sat at the console. They were told that they would hear a series of pairs of tones and that they were to judge each time whether the second tone of each pair was the same as, higher than, or lower than the first tone. The following tones were used in the test: CC = 32.7 d.v.; C♯C♯ = 34.6; DD = 36.7; D♯D♯ = 38.9; EE = 41.2; and FF = 43.6. These vibration numbers are approximate; they were computed by dividing those of the once accented octave by 8. The pairs were given in different chance orders and a series consisted of 36 pairs in which every note of the 6 followed itself and every other note once. The series was given 5 different times to every *O* on different days. Thus a total of 1620 judgments was taken, 180 for each *O*.

Distractions in this experiment consisted of street noise, mostly from street cars and trucks. Pairs which were not clearly heard on this account were repeated later in the series. There was also some accessory noise from the sounding of the pipes themselves but not of such a nature as would serve as a differential cue.

Before every experiment *E* gave a ready signal. Then the first tone was sounded for a period of 1½–2 sec., then after a pause of about 1 sec. the second tone was sounded for 1½–2 sec.

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[18] Opus 4991, installed in 1927 in the Wilson Memorial Methodist Episcopal Church, South, located at Charles Street and University Parkway in Baltimore.

[19] The organ had been recently tuned and the pipes were voiced so as to give within this range tones of approximately equal intensity.

All the *O*s knew that organ tones were being used for the test and hence that a discrete scale was being used. Hence there may have been some tendency to call tones that seemed less than a semitone apart the same, when a difference was actually perceived.

Results. The results from this experiment are shown in Tables IV–VII. Table IV gives for every *O* the total number of errors made each day, the

TABLE IV

NUMBER OF ERRORS MADE BY EVERY *O* ON EVERY DAY OF THE EXPERIMENT

	Observers							
Day	*A*	*B*	*C*	*F*	*H*	*L*	*M*	*N*
1	12	0	18	5	18	12	16	10
2	19	5	17	6	19	7	18	9
3	16	3	11	10	17	8	23	7
4	18	7	10	10	17	7	24	10
5	15	3	9	12	15	6	22	9
Total	80	18	65	43	86	40	103	45
% of all	44	10	36	24	48	22	57	25

total number of errors for the series, and the percentage that the total number of errors is of the total number of judgments. Every *O* gave 36 judgments a day, making a total of 180 for the series. There is not much variation in number of errors from day to day: the greatest is 9 (from 9 to 18 for *C*). *C* is the only one to show consistent improvement; *M* and *F* became more inaccurate and the others remain at about the same level from the

TABLE V

NUMBER OF EACH TYPE OF ERRORS MADE BY EVERY *O*

Type of Error	Observers							
	A	*B*	*C*	*F*	*H*	*L*	*M*	*N*
L judged as H	14	3	14	1	19	7	42	4
L judged as S	16	3	20	11	24	11	12	16
S judged as D	9	6	2	8	12	5	7	4
H judged as L	30	3	13	14	12	8	15	9
H judged as S	11	3	16	9	19	9	27	12

first day to the fifth. *B*'s errors are so few that they may be ascribed to attention lapses but the percentages of error of the others are formidable; those of *A*, *H*, and *M* are better than chance (67%) but still very large.

Table V shows the number of times each *O* judged the second tone of a HL pair to be higher or the same as the first, the number of times he judged the same notes to be different, and the number of times he judged the second tone of a LH pair to be the same or lower. Six *O*s (*A*, *C*, *F*, *H*, *L*, and *N*) judged the second note of a HL pair to be S a little more often

than H, and there is a like tendency for the LH pair. Since there were a total of 75 each of the HL and LH pairs in the series and 30 in which the same tone was given twice, Table VI was made up to show the percentages of each type of error. This table shows that except for *B* (and *F* to a certain extent) pairs of the same tones were judged more accurately than pairs of different tones, yet it is very significant that the same tone should be so

TABLE VI

PERCENTAGES OF EACH TYPE OF ERROR MADE BY EVERY *O*

Type of Error	Observers							
	A	*B*	*C*	*F*	*H*	*L*	*M*	*N*
L judged S or H	40	8	45	16	57	24	72	27
S judged H or L	30	20	7	27	40	17	23	13
H judged S or L	55	8	39	31	41	23	56	28

often judged to be different. Table VII gives the percentage of times that the same notes and notes one, two, three, four, and five semitones apart were judged erroneously. The percentages for the perfect prime are based on 30 judgments, for the minor second on 50 judgments, for the major second on 40 judgments, for the minor third on 30 judgments, for the major third on 20 judgments, and for the perfect fourth on 10 judgments.

TABLE VII

PERCENTAGE OF ERROR ON EACH INTERVAL MADE BY EVERY *O*

Interval	Observers							
	A	*B*	*C*	*F*	*H*	*L*	*M*	*N*
Perfect prime	30	20	7	27	40	17	23	13
Minor second	54	20	60	34	62	32	72	48
Major second	40	5	40	20	38	23	63	25
Minor third	43	0	33	17	50	23	60	13
Major third	50	0	25	20	40	5	45	10
Perfect fourth	50	0	20	10	50	20	80	10

B has a regular diminution of errors as the interval becomes larger; *C* and *N* have the most errors on the tones a semitone apart and a decrease as the interval becomes larger; the rest show little consistent change.

Summary and conclusions. This experiment shows that musically trained *O*s are much confused in their judgments of these low pitches. It reënforces the conclusion from Experiment I that pitch discrimination at these low ranges is much more difficult and erroneous than pitch discrimination in ranges above 64 d.v. The increase in difficulty and number of errors seems quite out of proportion to the short distance that the 32-44 range is below the range from about 64 upward, in which errors are never made in judging successive tones a half tone apart.

EXPERIMENT III

Experiment III is concerned with pitch discrimination near the upper tonal threshold.

Titchener gives a full account of the work done on the upper threshold up to 1905.[20] Since then studies on the upper threshold have been published by Helmholtz,[21] Gildemeister,[22] Pratt,[23] and Schlittler.[24] Lane computed the relation of intensity to audibility at extremely high pitches,[25] but Möller found variations in intensity above threshold intensity to have no regular effect on the tonal threshold.[26] Humes found that practice decreased the upper tonal limen and ascribed this to the *O*'s becoming more critical as the experiment progressed.[27] Apparatus for upper threshold work has usually been some form of the Galton whistle or Schulz monochord, both with their recognized limitations, and König's cylinders, which are difficult to calibrate.[28]

Apparatus and procedure. The procedure used was similar to that in Experiment II. The sixth accented octave on a set of König's cylinders was found to be within the threshold of audibility of all the *O*s and was used for the experiment. The bars used and the vibration numbers on them were: $c = 8132$, $d = 9216$, $e = 10240$, $f = 10922.67$, $g = 12288$, $a = 13653$, and $b = 15360$. *O* was seated on a stool 4 ft. from the cylinders; this was the distance used in the work of Möller, Humes, and others and was thought to be close enough for clear hearing and far enough away to eliminate cues from the spatial position of the cylinders. *O* was told that he would be given a series of tones and that he was to judge the second tone in each pair as higher, lower, or the same in pitch as the first. The pairs were made up so that each tone followed itself and each other tone once in each experimental period, and the pairs were given in chance orders. This amounted to 49 judgments a period, and as the test was given on 5 different days every *O* gave a total of 245 judgments. Twelve *O*s (*A, B, C, F, G, H, K, L, M, N, P* and *Q*) participated in this experiment.

E struck the bars with a steel hammer held in his right hand and damped them a little over a second later with a piece of felt held in his left hand so that the

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[20] Titchener, *op. cit.*, 41-46.

[21] W. Helmholtz, Vergleich zwischen der Galtonpfeife und dem Schulzeschen Monochord zur Bestimmung der oberen Hörgrenze, *Diss. Berlin*, 1910, 1-18.

[22] M. Gildemeister, Untersuchungen über die obere Hörgrenze, *Zsch. f. Sinnesphysiol.*, 50, 1919, 161-191.

[23] C. C. Pratt, Highest audible tones from steel cylinders, this JOURNAL, 31, 1920, 403-406.

[24] E. Schlittler, Die Bestimmung der oberen Tongrenze mittels der Galtonpfeife nach Prof. Karl Ludolf Schäfer, *Acta Oto-laryngol.*, 10, 1927, 81-89.

[25] C. E. Lane, Minimum sound energy for audition for tones of high frequency, *Phys. Rev.*, 19, 1922, 492-497.

[26] E. F. Möller, The effect of change of intensity upon the upper limit of hearing, this JOURNAL, 33, 1922, 570-577.

[27] J. F. Humes, The effect of practice upon the upper limen for tonal discrimination, *ibid.*, 42, 1930, 1-16.

[28] E. G. Boring, The construction and calibration of König cylinders, *ibid.*, 38, 1927, 125-127; S. W. Fernberger, An improved apparatus for actuating tonal cylinders of the König type, *idem*, 120-124.

duration of tones from all cylinders might be kept relatively constant. He attempted to keep the intensity of the strokes as constant as possible.

All the *O*s but *A* said that they liked this experiment but that it required close attention. *A* said that the tones hurt his ears, but many others said they sounded like sleighbells, etc., and were interesting. All the *O*s without exception reported that they based their judgments on the 'ring,' not on the 'knock.'

Results. Table VIII shows for every *O* the number of errors made every day, the total number of errors for the experiment, and the percentage that

TABLE VIII

NUMBER OF ERRORS MADE BY EVERY *O* ON EVERY DAY OF THE EXPERIMENT

Day	Observers											
	A	*B*	*C*	*F*	*G*	*H*	*K*	*L*	*M*	*N*	*P*	*Q*
1	17	15	9	10	10	10	18	5	21	24	10	26
2	14	11	8	4	5	14	12	7	19	20	7	27
3	14	10	9	6	8	11	12	5	24	28	19	21
4	13	13	5	7	10	9	15	2	24	20	22	19
5	6	11	7	6	10	17	19	7	20	30	11	19
Total	64	60	38	33	43	61	76	26	108	122	69	112
% of all	26	24	16	13	18	25	31	11	44	50	28	46

this total was of the total number of judgments. In general there is a certain level in number of errors maintained by every *O* throughout the experiment. *A* and *Q* are the only ones to show consistent improvement. If attention lapses are allowed for, the scores of *L*, *F*, *C*, and *G* could be considered very good, but the scores of the other *O*s point to a genuine diffi-

TABLE IX

NUMBER OF EVERY TYPE OF ERROR MADE BY EVERY *O*

Type of Error	Observers											
	A	*B*	*C*	*F*	*G*	*H*	*K*	*L*	*M*	*N*	*P*	*Q*
L judged as H	4	24	5	8	11	23	6	7	28	48	39	21
L judged as S	16	15	11	4	17	9	1	7	41	22	2	33
S judged as H	0	2	2	2	1	1	3	0	1	2	13	0
S judged as L	6	3	0	4	0	0	16	0	1	1	4	1
H judged as L	25	7	5	7	3	21	48	6	12	28	9	28
H judged as S	13	9	15	8	11	7	2	6	25	21	2	29

culty in discrimination. Table IX shows the number of errors of each type that every *O* made on the HL, LH, and same pairs; and since for each of the unlike pairs an *O* made 105 judgments and for the like pairs he made 35 judgments, in Table X is given the percentage that the number of errors of each type was of the total number of judgments on that type. Thus, 19% of the judgments *A* made on a HL pair were erroneous. This table shows

TABLE X

PERCENTAGE OF EVERY TYPE OF ERROR MADE BY EVERY *O*

Type of Error	Observers											
	A	*B*	*C*	*F*	*G*	*H*	*K*	*L*	*M*	*N*	*P*	*Q*
L judged S or H	19	37	15	11	27	30	7	13	66	67	39	51
S judged H or L	17	14	6	17	3	3	54	0	6	9	49	3
H judged S or L	36	15	19	14	13	27	48	11	35	47	10	54

that except for *F*, *K*, and *P* pairs of same tones are more often correctly judged than pairs of different tones. The three *O*s showing the reverse tendency probably built up in the course of the experiment criteria differing from the criteria of the others.

Pitch discrimination in this high octave is inaccurate for all the *O*s. *C*, *F*, *L*, and *G* show such good scores, however, that the hypothesis becomes plausible that after proper training the only errors that would be made on this test would be those due to attention lapses. But it is another question whether or not reactors could be trained to discriminate accurately differences much less than 1000 d.v. in this pitch region.

GENERAL CONCLUSIONS

Below 50 d.v. *O*s with pitch discrimination better than the average have great difficulty in telling which of two tuning-fork tones an octave apart is the higher; they are often forced into the use of secondary criteria and build up therefrom a system of wrong judgments. They do better with organ pipe tones from 32–44 d.v., but these have some overtones. Further work should be done on this problem when apparatus is available for producing absolutely pure tones from say 10 to 50 d.v. and when secondary criteria can be either eliminated or made of neutral effect. Final determination of the lowest objective vibration rate which can stimulate the cochlea must of course wait until tests are made with absolutely pure tones on people who have neither the tympanic membrane nor the ossicles. (This assumes that neither the external auditory meatus nor the tympanum would contribute a perceptible overtone differing for each low objective tone.) In persons with normal hearing the rate of flutter in low tones would always give a secondary criterion in judging low pitches, whether or not the tones are distinguishable from each other by their pitch characteristic alone. The only other approach to the final determination of the lower pitch threshold would be experimentation with *O*s whose sensory nerves leading from the membrane and ossicles had been anaesthetized or blocked in some way or other. The artificial production of this in many *O*s seems hardly feasible.

Nevertheless, the results of these experiments indicate that it is very doubtful that the cochlea itself responds to vibration rates slower than about 30 per sec.

Experiment III should be followed up by a learning experiment. If *O*s with more than ordinarily fine pitch discrimination in the middle register cannot learn to discriminate pitches of the diatonic scale above 8000 d.v. or even above 4000, this result would be difficult to explain on any current theory of audition.

THE COMPARATIVE EFFECTS OF SOCIAL AND MECHANICAL STIMULATION ON MEMORIZING

By JOSEPH PESSIN, University of Wisconsin

There have been a number of studies seeking to investigate the effects of mechanical stimuli, often called distractors, on various functions. The experiments of Evans,[1] Cassel and Dallenbach,[2] Morgan,[3] Tinker,[4] Hovey,[5] and Ford[6] may be mentioned. In general these studies have found that the initial effect of mechanical stimuli is disturbing, interfering with the efficiency of the particular function investigated. Later, individuals become accustomed to the mechanical stimuli, habituation sets in, and the function tested is as efficient as it was prior to the introduction of the so-called distractors.

The influence of social stimulation on various activities has also been investigated. An excellent summary of the earlier work is given by Allport,[7] and of the more recent work by Murphy.[8] No general statement of social influences on human activity can be made, since these influences vary with the nature of the social stimulus (a passive audience, a co-working group, a single spectator, etc.), and the activity investigated (simple sensori-motor tasks, cancellation, free association, etc.).

It seemed valuable to attempt an experiment which would test the relative effects of mechanical and social stimuli on the memory process. With this in mind, the present study was undertaken. It sought to incorporate both problems discussed above. We wished to determine whether social and mechanical stimulation influenced the memory process, and whether this in-

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[1] J. E. Evans, The effect of distraction on reaction-time, *Arch. Psychol.*, 5, 1916, (no. 37), 1-106.

[2] E. E. Cassel and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this JOURNAL, 29, 1918, 129-143.

[3] J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 5, 1916, (no. 35), 1-84.

[4] M. A. Tinker, Intelligence in an intelligence test with an auditory distractor, this JOURNAL, 36, 1926, 467-468.

[5] H. B. Hovey, Effects of general distraction on the higher thought processes, *ibid.*, 40, 1928, 585-591.

[6] A. Ford, Attention-automatization: an investigation of the transitional nature of mind, *ibid.*, 41, 1929, 1-32.

[7] F. H. Allport, *Social Psychology*, 1924, Chap. II.

[8] G. Murphy and L. B. Murphy, *Experimental Social Psychology*, 1931, Chap. 8.

fluence was favorable or unfavorable. Stated in terms of the function tested the purpose of this experiment is two-fold. It was undertaken to determine (1) the relative effects of social and mechanical stimulation on learning, and (2) the relative effects of social and mechanical stimulation on retention as measured by relearning after intervals of 1, 2, and 3 days.

The Effect on Learning

Material and apparatus. Three lists of nonsense syllables, seven 3-letter syllables in each list, were used. These syllables were presented one at a time for 1.5 sec. by means of Hull's exposure machine, improved model.[9]

The *S*s worked in a partitioned booth (5 x 5 x 8 ft.) which contained a small table and a chair. Directly in front of *S* were the machine's exposure windows which fitted snugly into a slit in the wall about 4 ft. above the floor. Above *S*'s head and out of line of vision was a board containing a 150-watt light and an electric buzzer. The light and buzzer were connected to a mercury metronome outside the booth. A curtained window about 3 ft. square was situated at *S*'s left. It was here that the social stimulus in the form of a spectator was introduced by lifting the curtain.

Procedure. Each S memorized lists of nonsense syllables under 3 conditions which we shall designate as mechanical, social, and control.

The control condition was one in which *S* worked alone in a partitioned booth unobserved by any individual and relatively free from extraneous stimulation. This may be a condition some would desire when engaged in serious mental work.

The mechanical condition was one in which *S* worked in the presence of combined visual and auditory stimulation. The visual and auditory stimuli consisted of the stimultaneous flashing of a 150-watt light and the sounding of a buzzer, introduced by means of a mercury metronome that beat 54 times per min.

In the social condition a spectator was introduced while *S* was memorizing. The writer acted as spectator for all *S*s. He remained entirely passive, making no attempt to offer verbal suggestion, encouragement, or reproach.

When *S* entered the experimental room he was conducted immediately to the partitioned booth and asked to be seated directly in front of the windows of the exposure machine. He was given the following typewritten sheet of instructions.

> This is not an intelligence test; we are not testing your capacity or ability. Our purpose in this investigation is to find some facts about learning. You are, therefore, requested to learn the material which will be presented on the exposure machine in front of you. You are to learn the syllables by anticipating them *aloud.* That is, before the syllable appears at the exposure window you are to pronounce it *out loud.* When the material is presented for the first time you will not be able to anticipate any syllables. But commencing with the second presentation you are expected to anticipate some syllables *out loud.* The material will be presented repeatedly and you are asked to anticipate the syllables *out loud,* until the exposure window is closed. Please do not read the syllable which is exposed; anticipate the syllable before it appears. If you do not understand the instructions, please read them again.

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[9] C. L. Hull, Quantitative aspects of the evolution of concepts, *Psychol. Monog.,* 28, 1920, (no. 123), 1-85.

Prior to actual experimentation, *S* was given a practice list of 7 syllables which he learned to the point of one perfect reproduction by the anticipation method. The results of the practice list were discarded and the material was not used in the experiment. *S* then learned one list of 7 nonsense syllables under control condition, a second under mechanical, and a third under social. The conditions were rotated so that none had any advantage because of temporal position. The lists were also rotated as is demonstrated by the learning program shown in Table I.

TABLE I

LEARNING PROGRAM SHOWING THE ROTATION OF THE CONDITIONS AND LISTS OF NONSENSE SYLLABLES

S	Learned		
	First	Second	Third
	List 1	List 2	List 3
1	Control	Mechanical	Social
2	Mechanical	Social	Control
3	Social	Control	Mechanical
	List 2	List 3	List 1
4	Control	Mechanical	Social
5	Mechanical	Social	Control
6	Social	Control	Mechanical
	List 3	List 1	List 2
7	Control	Mechanical	Social
8	Mechanical	Social	Control
9	Social	Control	Mechanical

The use of the above learning program may be explained by citing the first *S* as an example. *S 1* learned one practice list of 7 nonsense syllables to the point of one perfect reproduction. He then memorized List 1 under control conditions, List 2 in the presence of combined visual and auditory stimulation, and List 3 in the presence of a spectator. Each list was memorized first, second, and third by different *S*s and under all conditions of this experiment. The learning program controlled practice and fatigue effects, and balanced any inequality in the difficulty of the lists.

Subjects. Sixty college students recruited from the various classes were used. An attempt was made to obtain a random sampling of the university population. There were 36 men and 24 women.

Results. The results were scored in terms of errors and repetitions. The number of errors made and the repetitions required to reach the point of one perfect reproduction were computed. (An error consisted in failing to anticipate the correct syllable, anticipating the wrong syllable, or anticipating the correct syllable in the wrong order.) Table II shows the average number of errors and repetitions required to learn a list of nonsense syllables to the point of one perfect reproduction under the various conditions.

TABLE II

AVERAGE NUMBER OF ERRORS MADE AND REPETITIONS REQUIRED TO LEARN A LIST OF 7 NONSENSE SYLLABLES BY THE ANTICIPATION METHOD UNDER EACH CONDITION

(N = 60)

Condition	Errors	Repetitions
Mechanical	46.37	12.02
Social	41.10	11.27
Control	36.58	9.85

Table II shows that more errors were made in the mechanical situation than in either the control or social. The control situation was the most favorable for learning, inasmuch as fewer errors were made and fewer repetitions required when *Ss* worked alone undisturbed by noise or by a spectator. Ranked in order we may say that the control situation was best, the social next best, and mechanical worst. This is true both in terms of errors and repetitions.

The reliability of the differences is shown in Table III.

TABLE III

A COMPARISON OF THE VARIOUS CONDITIONS SHOWING DIFFERENCES AND CRITICAL RATIOS

(N = 60)

Conditions		Diff.	$S.D._{diff.}$	D/σ diff.	Chances in 100
Mech.-Soc.	Errors	5.27	3.19	1.65	95
	Repetitions	0.75	0.79	0.95	83
Mech.-Con.	Errors	9.79	3.21	3.05	100
	Repetitions	2.17	0.77	2.82	99
Soc.-Con.	Errors	4.52	2.72	1.66	95
	Repetitions	1.42	0.74	1.92	97

We may omit, temporarily, a comparison of the social and mechanical conditions, since we are principally interested in contrasting the results of the latter conditions with those of the control.

The table above shows that the difference between social and control conditions is not completely reliable, although the chances are 95 in 100, in terms of errors, and 97 in 100, in terms of repetitions, that the true difference is greater than zero. There can be little doubt however, as to the reliability of the differences between the mechanical and control conditions, since the critical ratio, in terms of errors, is 3.05. In terms of repetitions, the critical ratio between mechanical and control conditions is 2.82, there being 9974 chances in 10,000 of a true difference greater than zero.

Not all differences existing between the experimental conditions (mechanical and social) and the control are large enough to insure complete reliability, yet they are consistently in favor of the control condition.

Such consistent differences are taken as evidence that the control condition was more favorable for learning.

The conclusion just drawn is corroborated by an analysis of individual records. Each *S*'s performance was studied separately to determine which condition was most favorable for him. Each condition was ranked best, second best, and worst, depending upon the number of errors committed. The best condition contained the fewest errors, and the worst the most. Table IV shows the results in terms of errors.[10]

TABLE IV

THE NUMBER OF *Ss* WHOSE PERFORMANCE WAS BEST, NEXT BEST, AND WORST IN EACH OF THE 3 CONDITIONS INVESTIGATED, AS MEASURED BY THE NUMBER OF ERRORS

Condition	Best	Next Best	Worst	Equal	N
Control	31	19	8	2	60
Mechanical	13	16	28	3	60
Social	14	20	21	5	60

Of the 60 *Ss* who participated in this experiment, 31 or slightly more than 50% did their best learning under control conditions, and 8—approximately 13%—did worst. The performance of 19 *Ss* was next best. Learning under mechanical conditions shows a trend in the opposite direction; namely, fewer *Ss* learned best, and more poorest. The performance of 28 *Ss* was worst, that of 16 next best, and that of only 13 best under mechanical conditions.

The social situation appears to be more favorable for learning than the mechanical, and less favorable than the control. Fourteen *Ss* learned best, 20 next best, and 21 worst under social conditions. Of the 5 *Ss* who did equally well in two conditions, 3 were matched with the mechanical, and 2 with the control.

Analysis of the data in terms of group averages (Tables II and III) and individual records (Table IV) shows that learning nonsense syllables was more efficient when the *Ss* worked alone undisturbed by a spectator or noises and lights. These results do not agree entirely with the findings of Ford and Morgan. This may be due to the fact that the learning period in the experimental conditions was too short to permit the *Ss* to become accustomed to the external stimuli. It is possible, then, that our results measure the initial effects of external stimuli. A second factor of importance is that the nature of the task set before *S* differed in our experiment. Our *Ss* learned verbal material by the anticipation method. Ford's subjects

[10] The data were analyzed also in terms of repetitions, but this analysis is not presented here, since the results are substantially the same as for errors. For the same reason the data in terms of repetitions were omitted from Tables V and VI.

added silently single digits from a row of mixed letters, and wrote the answers, and Morgan's *S*s translated codes by pressing correct keys.

The Effect on Retention

Procedure. To determine the effect on retention of material learned under the various conditions of this experiment, the *S*s were divided into 3 groups and asked to return at intervals of 1, 2, and 3 days. All relearning occurred under the same condition as the original learning. Retention was measured by the Ebbinghaus Savings Method.

Results. In Table V the results of each group may be considered separately. Group 1 retained more of the material learned in the social and mechanical conditions than of that learned in the control condition. This

TABLE V

The Average Percentage Saved during Relearning in Terms of Errors

Condition	Group 1 After 1 day (N = 20)	Group 2 After 2 days (N = 20)	Group 3 After 3 days (N = 20)
Mechanical	43.82	54.67	51.65
Social	46.39	46.59	53.06
Control	36.59	40.43	37.57

is indicated by the greater average percentage saving during relearning. The various conditions may be ranked for retention value, first rank being assigned to the condition yielding the greatest average percentage saved during relearning. Such a ranking indicates that the social condition was first, the mechanical second, and the control third.[11]

This ranking is slightly altered by Group 2, the mechanical being first, the social second, and the control third. Of greater significance, however, is the fact that material learned under the mechanical or the social conditions is again better retained than that learned under control conditions. The results of Group 3 are similar to those of Group 1. The social condition was first, the mechanical second, and the control third.

The reliabilities of the differences between the various conditions in retention tests are presented in Table VI.

As in the original learning (see Table III), the differences are not large enough to insure complete reliability. If the material had been relearned under more favorable conditions, as in our control condition, the differences in retention between the experimental conditions and the con-

[11] Some retroactive inhibition doubtless affected the relearning, but it is considered a negligible factor since the retroactive inhibition affected all conditions approximately alike.

trol would doubtless have been greater.[12] Yet differences are evident, and in each case there is greater percentage gain for material learned in the experimental conditions. This points to the conclusion that material learned in the presence of a spectator or in the presence of noise and light is retained better than that learned in quiet conditions.

Also it is important to observe that the differences in percentage gained between the experimental and control conditions are greater after 3 days than after 1 day. This is seen by comparing Groups 1 and 3. For Group 3 the differences are: social minus control = 15.49%, and mechanical minus control =14.08%. For Group 1 the differences are: social minus control = 9.8%, and mechanical minus control = 7.23%. Two possible ex-

TABLE VI

A Comparison of the Various Conditions Showing Differences and Critical Ratios in Retention Tests

(In terms of errors)

Lapsed time	Conditions	Diff.	$S.D._{diff.}$	$D/\sigma_{diff.}$	Chances in 100
1 day	Soc.-Mech.	2.57	6.86	.37	64
	Soc.-Con.	9.80	8.71	1.13	86
	Mech.-Con.	7.23	8.23	.88	81
2 days	Mech.-Soc.	8.08	6.82	1.19	88
	Mech.-Con.	14.24	8.88	1.60	94
	Soc.-Con.	6.16	7.49	.82	79
3 days	Soc.-Mech.	1.41	6.30	.22	59
	Soc.-Con.	15.49	6.32	2.45	99
	Mech.-Con.	14.08	6.66	2.11	98

planations for the divergences in the two groups suggest themselves: (1) the fact that different *S*s were used in each group, so that the differences may be due to a sampling error. Such an explanation, however, does not seem plausible, since both groups showed approximately the same amount of saving in relearning under the control condition (Group 1 saved 36.59% and Group 3 saved 37.57%, a difference of less than 1%, which is negligible), indicating that there is some basis for comparing the two groups. (2) A second explanation is based on the fact that the influence of learning material under mechanical and social conditions becomes evident only after a lapse of time. Such influence is slightly evident in relearning after one day, but markedly noticeable after a lapse of 3 days. It is possible that some factors operate during learning in the experimental conditions which facilitate retention over a longer period. The better retention of material

[12] J. Pessin, The effect of similar and dissimilar conditions upon learning and relearning, *J. Exper. Psychol.*, 15, 1932, 427-435.

learned in the social and mechanical conditions may be attributed in part to the fact that slightly more repetitions were required in the original learning under these conditions. That retention is dependent upon the number of repetitions in the original learning has been demonstrated by Ebbinghaus,[13] Luh,[14] and others. The number of repetitions in the original learning does not, however, seem entirely adequate to explain the superiority in retention of the mechanical and social conditions. Although the mechanical condition required slightly more repetitions in the original learning than the social, the latter shows slightly superior retention after 3 days (see Tables II and V). It is, therefore, believed that other factors plus increased repetitions in the original learning are responsible for better retention of material learned in the experimental conditions. Perhaps the greater expenditure of energy, as evidenced by louder responses in the mechanical condition and shuffling about in the social, exerted in learning the material in the experimental conditions resulted in better fixation and consequently better retention.

Summary and Conclusions

The relative effects of mechanical and social conditions on learning and retention of nonsense syllables were studied. The results obtained seem to justify the following conclusions.

(1) That learning is more efficient under relatively quiet conditions than under conditions of mechanical or social stimulation.

(2) That nonsense syllables learned under mechanical and social conditions are better retained than similar material learned under control conditions. Also, that the percentage retained is greater after a period of three days than after one day.

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[13] H. Ebbinghaus, *Memory*, Eng. Trans. by H. A. Ruger and C. E. Bussenius, 1913.

[14] C. W. Luh, The conditions of retention, *Psychol. Monog.*, 31, 1922, (no. 142), 1-87.

INDIVIDUAL DIFFERENCES IN THE REPRODUCTION OF TEMPORAL INTERVALS

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An attempt was recently made by the writer to determine by the method of reproduction what is known as the temporal indifference interval.[1] According to the existing experimental literature on the reproduction of empty intervals bounded by instantaneous sounds, when stimulus-intervals shorter than a certain length are reproduced, the reproduction-intervals should be longer than the standard or stimulus-intervals, or, in other words, the average error in the reproductions should be positive. On the other hand, when the stimulus-intervals are long, the reproductions should be shorter, that is, should show a negative average error. The indifference interval—the length of interval at which the average errors of reproduction change from positive to negative—has varied enormously with the investigator. The study made by the writer, though extensive and thorough and conducted throughout under what appeared to be a careful and adequate control of conditions, instead of resulting in an accurate determination of the indifference-interval, as had been hoped, led to the conclusion that every length of interval, no matter whether long or short, was overestimated (made too long in reproduction) by some *S*s and underestimated by others. It is true that rather definite results were obtained as regards the *variability* in the reproductions with variation in the length of the standard or stimulus-interval, results which revealed, in particular, a tendency towards a minimum relative variability at 0.6 sec.; but as regards the *sign* and magnitude of the *S*s' average errors, the so called constant errors, there was so little uniformity that it appeared unjustifiable to average the results obtained with the different *S*s, as is customary, in order to determine the prevailing general tendency.

This outcome was unsatisfactory, for at least two reasons. In the first place, the absence of any general trend in the constant errors is hard to reconcile with the obtained rather definite and regular trend in the variable errors; and secondly, it leaves unsettled the cause of the differences

* Accepted for publication February 15, 1933.

[1] H. Woodrow, The reproduction of temporal intervals, *J. Exper. Psychol.*, 13, 1930, 473-499.

between individuals as also that of irregular changes noted in the constant errors of the same individual with different lengths of the stimulus-interval.

There are obviously a number of possibilities which should be further investigated. One of these is the possibility that groups of *S*s large enough to approximate adequate samples of the population might reveal uniformities not obvious in a group of only eight; another is the possibility that the effects of reproductions of one interval upon reproductions of another interval, whether such effects be those of practice, habituation, or contrast, carry over from one day to the next, but in different ways with different individuals.[2]

The present paper deals with two other possible influences upon the constant errors. The first of these is the production or non-production of a sound simultaneously with the taps by means of which *S* reproduces the stimulus-interval. The second factor, found to be of enormous importance, is the effect exerted by differences in the mental attitude of the *S*s, by which is meant differences in the way they went about their task of reproducing the intervals. This was studied by determining the effects of two sets of instructions which differed widely as regards the way in which the *S* was asked to attend to and to reproduce the intervals but which remained identical in calling upon the *S* to make his task that of accurate reproduction.

Apparatus and procedure. In the writer's former investigation of the reproduction of intervals, the apparatus was so arranged that, by means of certain electrical connections, at each tap of the *S* (made upon a key noiseless in itself) there occurred a sound exactly like the sounds used to bound the stimulus-interval. It seemed quite possible that the errors of reproduction would present a different picture if the *S*'s taps were unaccompanied by any noticeable sound. The apparatus and procedure, except for a slight change needed to render the taps noiseless, were the same as in the former investigation and therefore do not need detailed description.[3] The stimulus-intervals were bounded by instantaneous sounds. Each interval was preceded by a warning signal, the tone of a buzzer of 800 d.v. per sec., given approximately 2 sec. before the initial sound of the stimulus-interval and sounding for approximately 0.15 sec. Only one stimulus-interval was used on any one day. This interval was presented 50 times and reproduced once after each presentation. The time elapsing from the end of one reproduction to the warning signal of the next presentation of the stimulus-interval was usually about 12 sec., but varied

[2] A study which obviates both these possibilities by the use of nearly 300 *S*s divided into groups each of which groups is given but one interval, is now nearing completion. This study forms a part of a larger study under way in the Illinois laboratory for obtaining dependable, verifiable information concerning the fundamental facts about the errors in the comparison, absolute judgment, and reproduction of empty and filled temporal intervals.

[3] Cf. *op. cit.*, 476-484. In order to render the taps noiseless, key K5 was kept closed and relay R5 not used.

over a range of from 12–16 sec. The stimulus-intervals varied from 0.4–6.0 sec. and were controlled, if not over 1.0 sec., by a Bergstrom large model chronoscope, and if 2.0 sec. or over, by a Bowditch-Baltzar contact-clock. The reproduction intervals were measured by means of a Hipp's chronoscope controlled before and after each sitting for the particular stimulus-interval employed. The intervals used were changed from sitting to sitting, in regular order, from the shortest, at the first sitting, to the longest and then from the longest back, at the last sitting, to the shortest.

Instructions. The instructions were as follows.

You will first hear a "ready" signal; namely, the sound of a buzzer, and shortly after this signal, you will hear two clicks, separated by an interval of time. As soon as you have heard the two clicks, you are to reproduce the interval between them by two taps upon the key. The same interval will be given throughout the sitting. Try to inhibit any tendency to count and avoid making any movements of a rhythmical nature with your hands, feet, or vocal organs. Pay as little attention as possible to your breathing. Reproduce each interval independently, that is, no matter how long or how short it seems, reproduce it as it seems.

After *S* had read the instructions, he was asked to repeat them to *E*. Any point forgotten or misunderstood was gone over again. A uniform manner of tapping, with the fore and middle fingers pressing down together upon a Scripture noiseless key, was demonstrated. The fingers were never lifted entirely off the key, so that when the key was pressed down there was no noise due to the impact of the fingers upon the key. The tap was experienced simply through the skin and the muscles. The *S*s were asked to tap a number of times until they became accustomed to the key and the tapping movement, but were not given practice in the reproduction of the interval. After instructing *S*, *E* retired to a room, two doors removed, which contained the Hipp's chronoscope and all of the control-apparatus. The *S*s were advanced students in psychology. They were all unaware of the type of errors they might be expected to make. They were, of course, never informed concerning either the length of the stimulus-interval or their errors.

Results. The results obtained are summarized in Table I. Each constant error (C.E.) which appears in the table is the difference between the mean length of the stimulus-interval, as measured 10 times before and 10 times after the sitting, and the mean length of the *S*'s 50 reproductions. The variation in the mean length of the stimulus-interval (usually only several sigma) was negligible compared with the variation in the length of the reproductions, so that in place of the P.E. of each constant error, which in reality is the P.E. of a difference between two means, there has been used simply the P.E. of the reproductions. As in the case of reproductions by means of sound-producing taps, there is no regularity in the sign of the constant errors. In the entire table there are 21 positive C.E. and 21 negative ones. Both positive and negative errors occur with every one of the stimulus-intervals and these two kinds of errors are apparently distributed quite in a chance order. This result agrees perfectly with that previously obtained in the case of reproductions by sound-producing taps.

TABLE I

Constant Errors of Silent Reproductions of Empty Intervals

All intervals, P.E., and C.E. are given in sigma. N for each cell is 50; total N is 3,500. Below each C.E. (mean or average error), is given its P.E., preceded by a ± sign. Each given P.E. is the P.E. of the distribution, which is 7.1 times as large as the corresponding P.E.$_{C.E.}$.

Interval	Ss						Number of Values	
	Pn	*Ws*	*Fg*	*Sp*	*Bn*	*As*	+	−
Ascending Series								
400	−17 ±20	−40 ±25	−37 ±21	+29 ±20	+16 ±18	+16 ±22	3	3
600	+15 ±23	−18 ±25	−31 ±22	+81 ±45	+39 ±22	−4 ±22	3	3
800	+21 ±37	−3 ±40	−104 ±41	+40 ±39	+4 ±38	−55 ±33	3	3
1000	−60 ±52	+150 ±102	−80 ±51	+39 ±47	−46 ±70	−18 ±68	2	4
2000	−19 ±149	+107 ±334	−19 ±179	+96 ±174	−255 ±113	+137 ±221	3	3
4000	+673 ±390	−829 ±385	−869 ±375	−412 ±353	−1019 ±196	+274 ±501	2	4
6000	+1534 ±687	+269 ±779	+263 ±723	−762 ±794	−1237 ±324	+71 ±630	4	2
Descending Series								
6000	+1486 ±699	+972 ±909	−183 ±822	−187 ±747	−193 ±397	+33 ±682	3	3
4000	+1221 ±541	+379 ±423	−418 ±432	+58 ±437	−827 ±254	−16 ±412	3	3
2000	+43 ±185	−41 ±199	+17 ±155	−49 ±145	−2 ±97	−83 ±144	2	4
1000	+94 ±57	−45 ±92	−29 ±62	−34 ±40	+11 ±60	−63 ±66	2	4
800	+34 ±42	−62 ±37	−69 ±48	−102 ±41	+26 ±35	+3 ±44	3	3
600	−50 ±35	+32 ±30	−63 ±22	−31 ±35	+43 ±27	+1 ±29	3	3
400	−19 ±24	−32 ±16	−9 ±23	−33 ±30	+22 ±29	−14 ±17	1	5

The present results with silent reproductions also confirm the former ones, with reproductions by means of sound-producing taps, as regards the relation between variability and the length of the interval. In spite of the irregular shifts in the C.E., there is a marked and fairly regular relation between the variability, measured by the P.E., and the length of the interval. In general, individual differences in the P.E. are much less marked than those in the C.E., and there appears to be no good reason why the P.E. of different individuals should not be averaged, particularly as the general tendency revealed when they are so treated is substantially the same in the present study as in the preceding one, with 8 *Ss* ,who reproduced the intervals by sound-producing taps. In Table II are given in the columns headed Av. $P.E._{dis.}$ the averages of the absolute individual P.E. (shown in Table I), while in the columns headed Percent are given

TABLE II

THE RELATION BETWEEN VARIABILITY IN REPRODUCTIONS AND LENGTH OF STIMULUS-INTERVAL

Interval	Ascending series		Descending series		Av. percent	
	Av. $P.E._{dis.}$	Percent	Av. $P.E._{dis.}$	Percent	this study	earlier study
400	20.9	5.2	23.1	5.8	5.5	5.7
600	25.3	4.2	29.6	4.8	4.5	5.3
800	37.9	4.7	41.0	5.1	4.9	5.7
1000	65.0	6.5	62.8	6.3	6.4	5.8
2000	194.9	9.8	154.1	7.7	8.8	6.8
4000	366.7	9.2	416.3	10.4	9.8	11.1
6000	655.1	10.9	709.2	11.8	11.4	11.8

the relative variabilities, *i.e.* the averages of the individual P.E. expressed as percentages of the lengths of the stimulus-intervals. The results given in this table demonstrate that relative variability of reproductions is at a minimum when the interval reproduced has a length of 0.6 sec., and that this relative variability increases as the interval increases or decreases from this most favorable length. Of course the validity of this generalization is limited to the intervals studied. In particular, since the 0.6-sec. interval was the only one between 0.4 and 0.8 sec., it is impossible to determine whether the minimum relative variability occurs exactly at 0.6 sec., or at a slightly longer or shorter interval.

Since all the relevant *external* conditions were the same for all our *Ss*, the conclusion appears inescapable that the great variation between *Ss* was due to variation in the *internal* operations of the *Ss*—*i.e.* in the ways in which they went about their task. Significant change from time to time on the part of a given individual under supposedly constant external conditions is probably the rule in psychological experimentation. This fluctua-

tion in performance is decidedly pronounced when results from sittings scattered over a number of days are considered.[4] Consequently a study was undertaken of the effect upon the errors of reproduction of the processes occurring within the *S*s, a study of what may be termed the effect of attitude upon performance. The only way in which it was possible readily to obtain information concerning the attitudes of the *S*s was to study their introspections.

Like most investigators, the writer followed psychological tradition and asked for frequent introspective reports, and so by the end of the experiments had accumulated a large sheaf of manuscripts written by the *S*s. It is something of a problem just what to do with such material, when obtained as *accessory* to the study of the *S*'s performances. How often, as a sort of appendix to a quantitative study of some aspect of performance, we find tacked on several pages of introspective material, material which may constitute only a small selection, made by *E*, from a much larger mass of reports. It is difficult to understand how these introspective reports can be regarded as relevant to the accompanying behavior, unless it is possible to establish a correlation between the reports and the behavior. Relationship has to be *proved*, mathematically—here, the same as elsewhere —and not simply assumed. We know very little about such relationships, and yet many a psychological discussion may be found which has as its basis an assumed perfect correlation between the characteristics of introspectively reportable processes and those of overt behavior.

In this work on the reproduction of intervals, *E* carefully scanned the introspective reports of the *S*s but could discover only faint suggestions of a correlation between any features of the reports and the type of errors shown by the *S*s' performances. In such a situation, which is not an uncommon one, the chief use that can be made of the introspections is to regard them simply as affording suggestions for varied types of instructions, which, when formulated, must be tried out to determine whether there is any relationship between the different types of instructions and the ensuing behavior. Such a procedure, constituting what may be termed the method of differential instruction, was adopted. At first it led to no positive conclusions. Either the *S*s did not all follow the instructions or else the instructions required the adoption of attitudes that were irrelevant to the errors. Finally, and only in the case of one pair of instructions, was there obtained a really significant change in performance. According to one set of instructions, *S* was to listen to the sounds bounding both the stimulus-

[4] Woodrow, Quotidian variability, *Psychol. Rev.*, 39, 1932, 245-256.

interval and its reproduction interval as a pair of sounds and avoid strain; while according to the other set, he was to listen passively to the stimulus-interval but in reproducing he was to put his attention on the strain sensations which filled the interval between the bounding sounds. With change from one set of instructions to the other, there occurred an enormous change in the errors, a change which was not only of great magnitude but which was uniform in its direction. The 12 *S*s were each tested with both types of instructions and with two stimulus-intervals, 0.6 and 4.0 sec.

The first set of instructions was as follows.

Auditory—unanalytical—attention on the sounds.

(1) Stimulus. Attention solely on the sounds (in case of both stimulus and response). Listen to them as 'out there' somewhere, *i.e.* as going on outside of you and some distance away. A *pair* of sounds. Avoid any effort or strain; take it easy.

(2) Response. In reproducing the interval pay no attention to your finger movements—try to make them so automatically that you will hardly be aware of them. You simply reproduce the pair of *sounds*. Of course in reproducing the stimulus pair of sounds you are to make the interval between your two sounds exactly the same as that between the stimulus-pair; but do not analyze the whole into sounds and interval; with your attention on the sounds, make the whole "gestalt" *sound* just like the stimulus-gestalt. Do not look at the key you tap on: forget about the taps. Avoid any muscular tension, in hands, fingers, vocal or breathing organs. Avoid any humming or motor accompaniments. Forget your *self*.

The second set of instructions was as follows.

Strain—interval separated from the bounding stimuli—attend to the interval as such.

(1) Stimulus. Attention to the *interval* between the sounds, rather than the sounds.

(2) Response. Motor, *i.e.* get your attention immediately on your fingers and the two pushes you are to make on the key. Put your fingers and your body, breathing and vocal muscles, on a strain while reproducing the interval. Say hm . . . .m to yourself and keep your attention on these various movements. Be sure to tense your fingers. Try to keep your mind so occupied with attention to your muscle experiences that you are scarcely aware of the sounds resulting from your taps. Be sure to make the interval between the two pressings of the key exactly equal to the stimulus-interval.

Perhaps better terms than 'auditory' and 'strain' for distinguishing between these two sets of instructions would be the terms 'extrovert' and 'introvert.' Such designations would also have the merit of preventing these instructions from being confused with the 'sensory' and 'motor' instructions sometimes used in work on reaction-times.

On any one day only one interval and one set of instructions were used, and as a rule several days elapsed between any two sittings. The trials with one interval were completed before going to the other interval, but the order in which the two instructions and the two intervals followed each other varied among all the possible orders.

E of course had no way to make sure that the rather complex instructions were carried out. The *S*s, however, were either advanced students or members of the

teaching staff in the department of psychology and intelligent coöperation could be assumed. All of the 12 *Ss* had had previous experience in introspection. They all stated that they understood the instructions and that they thought they had carried them out. Their reports indicate that, in the words of *Cr*, "factors mentioned in the instructions dominated the set;" but additional factors not mentioned in the instructions, the influence of which, if any, is unknown, also came into play. Many of these additional factors are probably inconsequential. On the other hand it is quite possible that some of them greatly modified the effect which the instructions would otherwise have exerted. One such factor, for example, may be that indicated by reference on the part of a few of the *Ss* to patterns and groupings, to swings of a pendulum followed by the eyes, and the use of the word rhythm, usually in a vague manner. Such references are well illustrated in the reports of *Em*, extracts from which are as follows.

Auditory instructions, 0.6 sec. "The two sounds were similar to time-beats in music and their reproduction a matter of continuing the rhythm. The whole four sounds formed one phrase, including the time between the two pairs of sounds."

Strain instructions, 0.6 sec. "Concentrating on the finger-movement, the tick-tocks were not so clear, and a definite finger pattern became evident. In reproducing the interval I was conscious of the same experience as in practicing five-finger exercises upon the piano—a definite pattern of down and up movements in the fore-finger."

Auditory instructions, 4.0 sec. "These longer intervals were not so rhythmical as the shorter ones and did not form a pattern of stimulus-wait-response."

Strain instructions, 4.0. sec. "The intervals tended to form a pattern of stimulus and response, but not so clearly as with the shorter intervals."

These introspections are in accord with the well-known fact that widely separated sounds do not readily enter into a rhythmical grouping, but their bearing on the sign of the C.E. at any given interval, is an exceedingly difficult and entirely open question.

The *Ss*' reports under these instructions afford positive information on several interesting matters. One of these is the question what strains were used in complying with the "strain" instructions. The strains reported varied with the *S*, but were most frequently derived from the fingers, hand or arm, or from the vocal or respiratory organs. Other parts of the body mentioned as the seat of strain were "the back of the throat," "the vocal cords," "the wrist," "muscles involved in gritting the teeth," the "whole body," and the "viscera."

Em wrote that "although there was strain throughout the whole body, it was particularly noticeable in breathing and the right arm;" at another time, "the strain was localized in the fore-finger rather than throughout the arm." Two *Ss* mentioned an increase in the intensity of the strain towards the end of the interval; 4 *Ss* said the strain was stronger or more vivid during the response interval than during the stimulus-interval, while none reported the reverse to be true. Two *Ss* said that the motor pattern was not the same during the stimulus and the response intervals. Thus *Cr* reported: "I exhale in one and inhale in the other." *Dg* (4.0 sec. interval, strain instructions) referred to extreme nervousness of his fingers and stated that

"just before the time to give the second sound my fingers shook considerably until all at once they became relaxed and I pushed the key." Several of the *S*s reported that it was harder to fill the short interval with strain than to do so in the case of the long one. *Cr* (0.6. sec. interval, strain instructions) wrote: "I often failed to notice any organic quality until both the [stimulus] clicks were over, so I tried to notice my breathing while the clicks occurred and then tried to reproduce in terms of breathing. Easiest was to say click-click and then reproduce the same muscular pattern. That seemed the 'natural' way for me. Unconsciously I seem to beat time somewhere in the body of these quick clicks and to reproduce that pattern." Similarly, *Mr* found the strain instructions harder with the short interval. He wrote that "the short interval does not have the 'timeyness' of the longer one. There is, however, a short going-on, although I can't say what it is that goes on. The body is on a strain, but very little happens to that strain. There is strain and that is all, but in the longer interval there is quite a pattern to the strain—increase in intensity and pain that means fatigue. When the going-on is there, there is a downward pressure which is neither bright nor dull nor heavy nor light, localized where I believe my stomach is. There is very little of this down-pressure with this short interval." *Rk* described his experience of the 0.6-sec. interval, under strain instructions, in the following vivid manner: "Between the clicks there was a dull thud as though a man were making a short jump in between two jumps."

From these introspections it appears that an attempt to attend to the interval as separate from the sounds leads only to the observation of strain. *Mr,* however, referred to a quality designated by the neologism "timeyness"; and several *S*s referred to experiences of waiting and of expectation without observed strain.

Even with auditory instructions, the *S*s sometimes mentioned strain or motor experiences, but in this case, they tried not to attend to them.

Thus *Cr* wrote as follows, after reproducing the 4.0-sec. interval with auditory instructions: "Hard to keep attention off the organic factors. I tried out various things such as taking one posture in listening and another in reproducing or changing my breathing voluntarily, so as to make it difficult to be guided by organic factors. After a while, however, I took it more casually and in many cases didn't notice at all the organic factor. The two sounds were the dominant factor." On the other hand such reports as the following were made concerning the effect of the auditory instructions. *Em,* (0.6 sec.) wrote that "there was no strain of any sort, nor any effort of listening."

One other point is perhaps worthy of mention; namely, the involuntary character of the *S*'s control of the length of the reproduction interval. Scores of reports on this matter, distributed throughout all parts of this work on the reproduction of intervals, have been obtained. The following are some illustrations.

Rb (0.6. sec., strain instructions): "My arm seems to give the second tap of its own accord. I set off the first tap, but the second takes care of itself."

Dg (4.0 sec., auditory): "There was a feeling of a growing nearness of action, then it was as if something said 'now' and I pushed the key the second time."

Cr (4.0 sec., auditory): "The second tap seemed to come inevitably at a certain point."

Em (0.6. sec., auditory): "The intervals were more or less unconsciously reproduced."

Of course it might be said that we have no sense of initiating most of our movements; they simply take place. However that may be, many of the *Ss* seemed impressed by the fact that once they set themselves to reproduce an interval, the reproduction took place in a satisfactory manner without their knowing why or how. What particularly impressed them was that while the reproduction-interval seemed to be initiated at an intended instant, the completion of the interval took care of itself.

The effect of the two sets of instructions upon the errors of reproduction are summarized in Table III. It will be observed that the reproductions with strain instructions averaged much longer than those with auditory instruc-

TABLE III

THE EFFECT OF DIFFERENCE IN INSTRUCTION UPON THE CONSTANT ERROR OF REPRODUCTION

Intervals, C.E., and P.E. are given in sigma. The P.E. given is the P.E. of the distribution (7.1 times the $P.E._{C.E.}$). N for each C.E. is 50; total N is 2,400.

Stimulus-Interval	S	Instructions: Auditory (A) C.E.	Auditory (A) P.E.	Strain (S) C.E.	Strain (S) P.E.	Difference in C.E. (S−A)	$D/P.E._{diff.}$
600	*Cr*	−2.3	27	+71.4	64	+73.7	+7.5
	Mr	−30.6	25	+15.8	24	+46.4	+9.4
	Rk	−22.8	32	+55.3	78	+78.1	+6.6
	Mn	−36.6	41	+21.2	32	+57.8	+7.6
	Ln	+0.4	33	−10.0	24	−10.4	−1.8
	Em	−14.4	18	+18.9	26	+33.3	+8.9
	Kp	−15.6	35	+36.0	50	+51.6	+7.3
	Ds	−94.2	77	−20.1	58	+74.1	+5.1
	Dg	−12.2	30	+9.5	26	+21.7	+3.8
	Fl	−7.0	34	+51.0	35	+58.0	+8.3
	Ke	−20.4	25	+24.3	30	+44.7	+6.2
	Ht	+17.7	28	+33.1	39	+15.4	+3.6
	Av.	−19.8	33.8	+28.3	39.5	+48.1	+6.0
4000	*Cr*	−129.8	429	+373.7	287	+503.5	+6.9
	Mr	−1787.6	213	−196.1	527	+1591.5	+19.7
	Rk	−515.7	444	+246.7	479	+762.4	+8.2
	Mn	−109.0	297	+1155.9	674	+1264.9	+12.1
	Ln	+49.0	406	+338.3	294	+289.3	+4.0
	Em	−766.5	198	−243.1	279	+523.4	+10.0
	Kp	+117.6	479	+408.0	307	+290.4	+3.6
	Ds	−472.6	696	−135.6	493	+337.0	+2.8
	Dg	−809.1	327	−512.1	296	+297.0	+4.8
	Fl	−619.1	398	+461.8	409	+1080.9	+13.4
	Ke	−166.1	327	+689.2	394	+855.3	+11.5
	Ht	−290.0	421	+475.5	478	+765.5	+8.2
	Av.	−472.1	386.3	+274.0	410.0	+746.1	+8.8

tions. In the case of the 0.6-sec. interval, the strain reproductions averaged 48.1 σ longer than the auditory reproductions, and in the case of the 4.0-sec. interval they averaged no less than 746.1 σ longer. It is true that the strain instructions did not always cause 'overestimation' nor the auditory instructions 'underestimation.' In 23 out of the 24 cases, however (the only exception being *Ln* with the 0.6-sec. standard), the strain reproductions were longer than the auditory reproductions—shown in the table by the fact that the values in the column headed 'Difference in C.E. (S-A)' were positive in all cases but one. Furthermore, the difference in the length of the reproduction was, in the case of the great majority of the *Ss*, statistically significant, averaging for the individual *Ss* over 6 times the P.E. of the difference in the case of the 0.6-sec. interval and over 8 times the P.E. of the difference in the case of the 4.0-sec. interval. If the results for all *Ss* were pooled into one distribution, in the case of each interval the difference between the two averages (in terms of the $P.E._{diff.}$) would be even greater.

This result is, of course, only a beginning. The two sets of instructions were admittedly complex. To determine what were the really effective elements in them would require much further work. Moreover, from the mass of introspections obtained, it is obvious that the varieties and subvarieties of attitudes which *Ss* may take in even such a simple experiment as the reproduction of the interval between two clicks is bewildering. Probably fluctuation from one to another of many of these attitudes has no bearing upon the nature of the errors of reproduction. On the other hand, it is not contended that the difference in attitude produced by the difference between the instructions described above is the sole difference in the attitude between *Ss* who 'overestimate' and those who 'underestimate.' Indeed, it seems certain that this could not be the case, since the one set of instructions did not always cause overestimation and the other underestimation. Moreover, if the average errors (C.E.) of a large group of *Ss* are plotted, the distribution will be smooth, with a considerable frequency (though not usually the modal frequency) at zero error. What the present experiment proves is simply that variation in attitudes is extremely influential and may be sufficient in some cases to determine whether an *S*'s reproductions will show great overestimation or great underestimation. It affords experimental support of the conclusion that in the reproduction of empty intervals the nature of the errors depends in large part upon the attitude of the *Ss* toward their task. To determine to what extent the nature of these errors depends also upon the length of the interval requires an investigation planned in other fashion than those hitherto made.

AN OBJECTIVE EXPERIMENT ON REASONING

By S. N. F. CHANT, University of Toronto

The purpose of this investigation was to analyze by a method of continuous observation some of the characteristics of the reasoning process.

Method and procedure. The materials were eleven 5 x 8 cards on each of which was drawn a fictitious coat of arms. Each coat of arms was composed of 6 separate drawings. Beside these were printed 6 items of information concerning the family to which the coat of arms supposedly belonged, and each drawing in any particular coat of arms stood for one of the items of family information printed on the same card. No rules of heraldry were followed. In all there were 14 drawings which were repeated in different groupings, along with their respective items of family information, throughout the series of 11 cards. The complete series, with Card 1 shown as a sample and the remainder with the drawings indicated by name instead of diagrammatically, was as follows:

Card 1

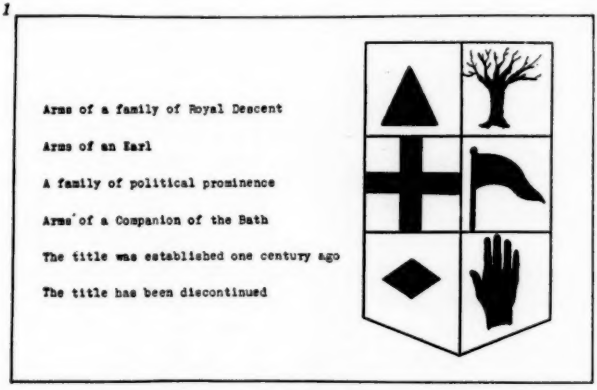
Arms of a family of Royal Descent
Arms of an Earl
A family of political prominence
Arms of a Companion of the Bath
The title was established one century ago
The title has been discontinued

FIG. I. CARD 1.
(A sample of the material used in the experiment.)

Card 2

Sword	2 Triangles	Arms of a family of Royal Descent. Arms of a family of Scotch origin.
Tree	Bar	Arms of a Baron. A family of scientific prominence.
Flag	Snake	The title was established 2 centuries ago. The title has been discontinued.

* Accepted for publication June 1, 1931. Reported at the 1930 (Iowa) meeting of the American Psychological Association.

Card 3		
Snake	Flag	Arms of a family of Scotch origin. Arms of a family of noble descent.
Cross	Rose	Arms of a Baron. A family of political prominence.
3 Triangles	Sword	The title was established 3 centuries ago. The title has been discontinued.
Card 4		
Sword	Cross	A family of noble descent. Arms of a Baron.
Anchor	Flag	A family of seamen. A family of political prominence.
Rose	4 Triangles	The title was established 4 centuries ago. The title has been discontinued.
Card 5		
Fish	4 Triangles	Arms of a family of Scotch origin. Arms of a family of noble descent.
Snake	Cross	Arms of a Viscount. A family of political prominence.
Rose	Flag	The title was established 4 centuries ago. The title has been discontinued.
Card 6		
Snake	Hand	Arms of a family of Scotch origin. Arms of an Earl.
Flag	Diamond	A family of commercial prominence. A Companion of the Bath.
Elephant	Triangle	The title was established one century ago. The title has been discontinued.
Card 7		
Cross	Snake	Arms of a family of Scotch origin. A family of noble descent.
Hand	4 Triangles	Arms of an Earl. A family of commercial prominence.
Flag	Rose	The title was established 4 centuries ago. The title has been discontinued.
Card 8		
Cross	Sword	Arms of a family of noble descent. Arms of a Baron.
Flag	2 Triangles	A family of political prominence. A family of literary prominence.
Rose	Bell	The title was established 2 centuries ago. The title has been discontinued.
Card 9		
Rose	Hand	Arms of a family of noble descent. Arms of an Earl.
Elephant	Anchor	A family of commercial prominence. A family of seamen.
Triangle	Flag	The title was established 1 century ago. The title has been discontinued.
Card 10		
Elephant	Cross	Arms of a family of noble descent. Arms of a Baron.

Rose	Flag	A family of political prominence. A family of commercial prominence.
Sword	3 Triangles	The title was established 3 centuries ago. The title has been discontinued.

Card 11

Bell	Flag	Arms of a Baron. Arms of a Companion of the Bath.
Sword	Diamond	A family of literary prominence. A family of seamen.
Anchor	3 Triangles	The title was established 3 centuries ago. The title has been discontinued.

The *S*s, 69 in number, were all undergraduates who had completed an introductory course in psychology. Each *S* was observed individually while he worked at the problem for one half hour. The 11 cards, an answer sheet, and the following printed instructions were provided each *S*.

On each of these cards is a coat of arms composed of 6 separate drawings. On each card are also 6 items of information concerning the family to which the coat of arms belongs. Each drawing on any particular coat of arms stands for one of the items of family information given on the same card. You are to determine which item of information each drawing stands for. You may use the cards in any manner you wish.

On the record sheet in column 1, name or make a rough sketch of the drawing from the coat of arms. In column 2 record opposite each drawing the particular item of family information it stands for. In column 3 record at the same time as you answer your reason for giving your answer. You may correct any answer at any time by marking it "w," and recording the correct answer.

Two *S*s out of 69 misunderstood the instructions and their results are not included.

A continuous record in 15-sec. intervals was made of the *S*'s behavior while working at the problem. This record was made on a sheet that was ruled into 120 columns to serve as time divisions, and into 7 horizontal divisions to provide sections for recording 7 characteristic types of behavior in this setting, which were determined by preliminary experimentation. The 7 possible records were as follows: Recording by number (1) any cards examined individually; (2) any two or more cards compared; (3) any cards examined in serial order; (4) any cards and answers compared; (5) the answers as they were written; and recording (6) any apparently random examination of the cards; and (7) any apparent periods of inaction.

The timing of those records was arranged as follows. A rack and pinion, operated by the ratchet wheel of the line-spacer of a typewriter, moved a vertical metal strip across the record sheet the distance of 1 time division whenever the spacing lever was pressed. In *E*'s line of regard when observing the *S* was an Eastman Timer. The record was made by placing a number or check in the appropriate behavior division of the time column, as indicated by the metal strip. Every 15 sec., as shown by the timer, the spacing lever was pressed and the metal strip moved forward one time division. After some practice the record could be made without diverting *E*'s attention from *S*.

Results. The method of recording described above made possible an analysis of the time spent by *S* in each of the 7 types of behavior recorded, as well as a detailed analysis of the behavior that preceded each answer given. Only 3 of the

several possible time-analyses of these data are required for purposes of this study, namely: (1) the time devoted to the overt comparison of cards with cards, or of cards with answers, including sections 2, 3, and 4 of the record outlined above; (2) the time spent in the examination of individual cards, *i.e.* section 1 of the record; and (3) the time spent in recording answers, *i.e.* section 5 on the record.

Fig. 2 shows the distribution of cases for time spent in comparison in 15-sec. time-units. Note that the first class interval extends only from 0 to 1.

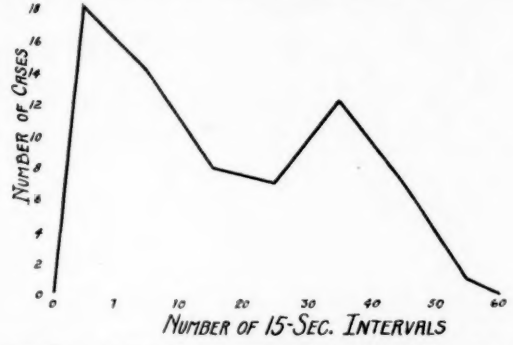

FIG. 2. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN COMPARING THE CARDS AND THE ANSWERS

FIG. 3. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN EXAMINING THE INDIVIDUAL CARDS

Fig. 3 gives a similar distribution of cases for time spent in the examining of individual cards.

Fig. 4 gives a similar distribution of cases for time spent in recording answers.

The bimodal character of the distributions shown in Figs. 2 and 3, as compared with the more normal shape of that shown in Fig. 4, indicates that with reference to both time spent in comparison and time spent in the examination of individual cards, the *Ss* can be divided into an upper and a lower group. That this

division is a function of the variables dealt with, and not common to all time studies in this experiment, is shown by the fairly normal nature of the distribution for the time spent in recording answers.

That the upper modal group in one distribution corresponds roughly to the lower modal group of the other distribution is evidenced by the negative coeffi-

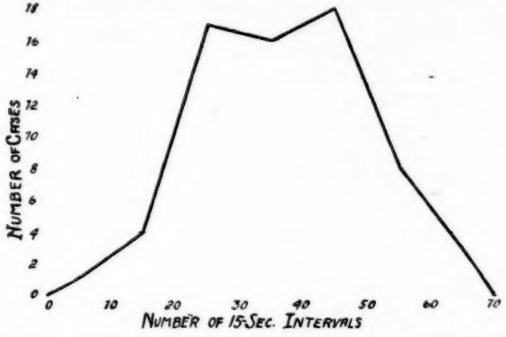

FIG. 4. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN RECORDING ANSWERS

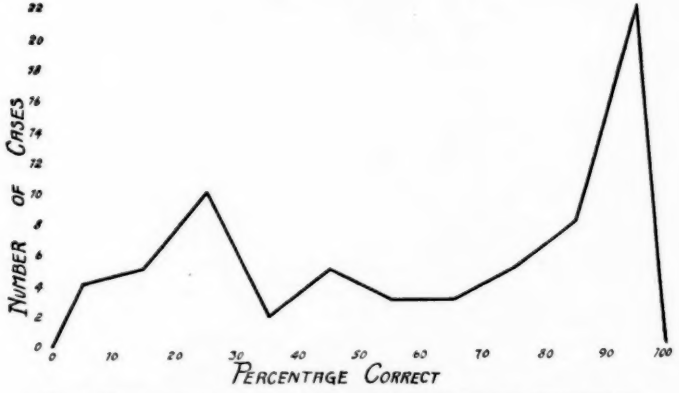

FIG. 5. DISTRIBUTION OF THE REPORTS ACCORDING TO THE PERCENTAGE OF CORRECT ANSWERS

cient of correlation, –0.86, P.E. 0.02, between time spent in comparison and time spent examining individual cards. The *S*s therefore can be divided into two groups: (1) those who devote considerable time to the comparison of cards or of cards and answers and little time to the examination of individual cards; and (2) those who devote little or no time to comparison and relatively much time to the examination of individual cards.

Fig. 5, giving the distribution of scores in terms of the percentage of correct answers to the number of answers given, shows a bimodality similar to that seen in Fig. 2 and 3. This suggests a relation between the answers given and the distribution of time.

It is obvious from the nature of the experimental material that correct answers can be obtained rather easily by comparison. Answers obtained in any other manner may not be correct.

TABLE I

SHOWING THE ANSWERS GIVEN FOR THE DRAWINGS ON THE FIRST CARD BY 21 Ss

Drawings	Answers					
	Royal Descent	Arms of an Earl	Political Prominence	Companion of Bath	Antiquity of Title	Title Discontinued
Triangle	2	8	0	2	3*	6
Tree	17*	0	0	0	2	2
Cross	2	9	2*	4	1	3
Flag	0	3	11	3	1	3*
Diamond	0	1	4	1*	11	4
Hand	0	0*	4	11	3	3

* Indicates correct answers in each instance.

Table I, showing the answers given for each drawing of the first coat of arms by *Ss* who had examined no other card previously to recording these answers, indicates that these answers are not merely a chance arrangement. Accordingly there is some factor other than chance that is operative in determining answers not derived by comparison. The basis for these answers can be found in the explanations given by the *Ss*. To illustrate, the reasons are listed below for the 4 instances shown in Table I in which over half of the *Ss* agree in assigning the same answer to a particular drawing, namely: (1) the 17 answers in agreement that are correct for tree; (2) the 11 answers in agreement that are incorrect for flag; (3) the 11 answers in agreement that are incorrect for diamond; (4) the 11 answers in agreement that are incorrect for hand.

(1) Reasons given for assigning, 'Arms of a family of royal descent' to tree, which is correct:

It is a family tree (5 times).
The tree is a royal oak (3 times).
A tree stands for descent (twice).
A family tree representing descent (twice).
Royal Families can trace their descent, hence a tree.
The tree has many branches like the Royal Family.
The tree is deeply rooted like the Royal Family.
The tree is firmly rooted in the land like the Royal Family.
The tree is an oak indicating uprightness, strength and age; the Royal Family would be stronger, older and above any other family.

(2) Reasons given for assigning, 'A family of political prominence' to flag, which is incorrect:

Flag stands for patriotism (3 times).
Flag stands for the country (3 times).
Banner signifies leaders of political party.
Banner is a political symbol.

The banner would be waving for everyone to see.
The flag is the symbol of the country.
The only inference left.

(3) Reasons given for assigning, 'The title was established one century ago' to diamond, which is incorrect:

The diamond stands for a century (3 times).
Only answer left (3 times).
Conventional sign for 100.
Diamond sign of age.
The diamond has 4 quarters like a century.
The diamond represents a complete whole like a century.
The diamond has completed sides like a century.

(4) Reasons given for assigning, 'Companion of the Bath,' to hand, which is incorrect:

The hand stands for comradeship (4 times).
The hand suggests washing (twice).
The hand indicates a companionable order.
It is the hand of comradeship.
It is the helping hand of comradeship.
The hand is used in bathing.
It is the mailed fist of a military order.

These examples, which could be multiplied many times, indicate that in the great majority of instances answers derived without comparison are based upon previously established associations.

For purposes of comparison the following reasons are listed which were given by all *Ss* assigning the correct answer to hand, which is 'arms of an Earl.'

By comparing cards 1, 6, 7 (22 times).
By comparison (5 times).
By comparing cards 1, 6, 7, 9 (3 times).
By elimination.
A hand stands for the power of an Earl.

Here only one answer out of 32 is based upon a previously established association, the other 31 being based upon an analysis of the experimental material by means of comparing various cards.

The answers, therefore, on the basis of the reasons assigned for giving them, can be divided into (1) those based upon previously established associations; and (2) those derived by an analysis of the experimental material by means of comparison. Similarly the *Ss* can be divided into 2 groups: (a) 32 *Ss* who gave predominantly answers based upon previously established associations. In this group are 26 *Ss* who gave only answers based upon previously established associations, and the smallest percentage of such answers given by any one *S* in this group was 64% of all the answers given by that *S;* and (b) 35 *Ss* who gave predominantly answers based upon the analysis of the experimental materials by comparison. Twenty-five members of this group gave not more than one answer each based upon previously established associations, and the smallest percentage of answers derived by comparison given by any one *S* in this group was 78% of the total answers given by that *S*.

That this division of *S* agrees remarkably well with the time analysis given above in Figs. 2 and 3 is shown by the superimposed graphs shown in Figs. 6 and 7.

Fig. 8 shows the 67 *Ss* in an array based upon the time spent in the examination of individual cards, shown by the strokes. The time spent by each *S* in the comparison of cards and answers is shown by the dashes. The numbers along the base line show the division of *Ss* based upon the nature of their answers: 1 indicates that the *S* belongs to the group which derives answers from previously established associations; 2 indicates that the *S* belongs to the group which derives answers by comparison.

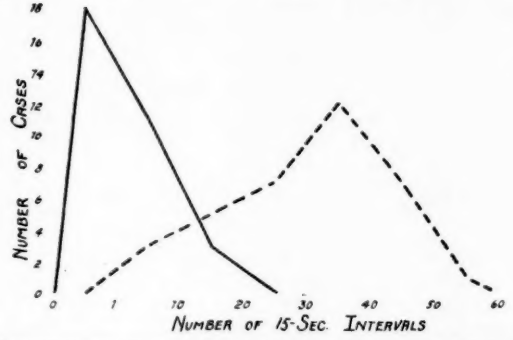

FIG. 6. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN COMPARING CARDS AND ANSWERS
——— = Answers based upon previously established associations.
- - - - = Answers based upon comparisons.

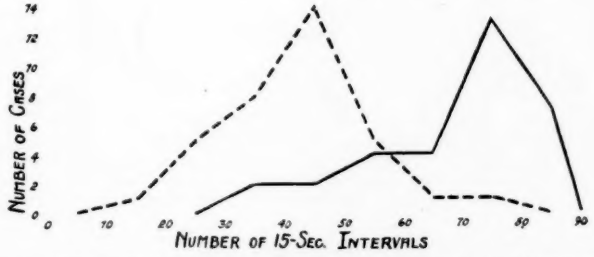

FIG. 7. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN EXAMINING THE INDIVIDUAL CARDS
——— = Answers based upon previously established associations.
- - - - = Answers based upon comparisons.

The pronounced agreement between the division of the *Ss* on the basis of their answers and the time studies justifies the division of the *Ss* into (1) those who emphasize almost entirely the examination of individual cards and derive their answers from previously established associations, and (2) those who emphasize the comparison of cards or cards and answers and derive their answers through an analysis of the experimental materials.

Conclusions. The above division of Ss clearly supports the conclusion that there are two well-defined methods of reasoning exemplified in this setting. The first is an interpretative approach, by means of which answers are derived from previously established associations that are remote from the experimental setting itself. The second is analytical, by means of which answers are derived by comparing cards or cards and answers and therefore within the experimental setting.

The first of these, the interpretative, appears to be the more elementary or basic type, while the second, the analytical, appears to be largely a product of training, as indicated by the three following evidences:

(1) One drawing was intentionally introduced into the experimental material which obviously was related to its particular item of information by a very com-

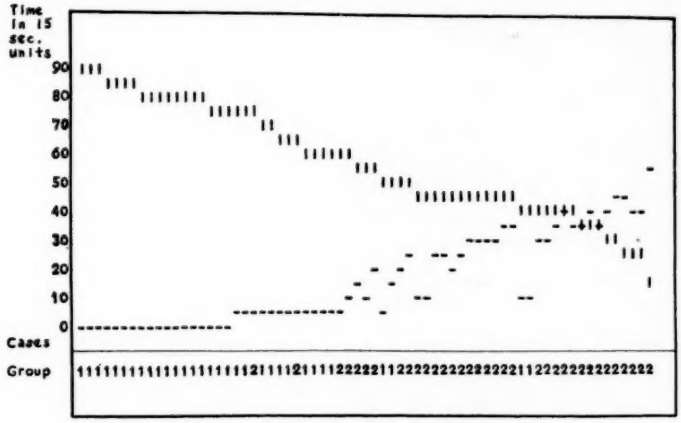

FIG. 8. SCATTERGRAM OF 67 CASES

Time spent in examining individual cards indicated by vertical lines; time spent in comparing cards and answers indicated by horizontal lines; answers based upon previously established associations designated by 1; answers based upon comparisons designated by 2.

mon previously established association; namely, an anchor standing for a family of seamen. This drawing appeared on cards 4, 9, and 11. The time-records show that 77% of the group employing comparison forsook the method of comparison in this case and derived the answer from some previously established association, whereas an examination of the records shows only 31% of this group giving even one other answer that is not preceded by comparison. Hence there is a tendency to revert from the analytical to the interpretative approach when the previously established association is so obvious.

(2) The errors made by the group employing comparison show the influence of previously established association in that, as a rule, they are in agreement with the answers given by the first group. For example, assigning 'Order of the Bath' to hand and a 'Family of Scotch origin' to sword accounts for 45% of all the errors

of the group employing comparison, and these are the most frequent answers given by the group employing previously established associations, in both instances.

(3) The third evidence for this conclusion is that 87% of the group employing comparison have studied some experimental science in addition to psychology, whereas only 42% of the group depending upon previously established associations have done so, which indicates that the analytical approach is probably the product of academic training along scientific lines. This is the only striking difference to be found with reference to the content of courses taken by these *S*s, nor did one group appear to obtain better grades than the other in any courses with any degree of consistency.

Since this experiment was undertaken as an initial attempt to investigate by objective methods the nature of scientific inference, the writer feels justified in adding the following brief note to be considered more thoroughly after a further study of the entire problem. Both of the approaches indicated in this investigation find a place in scientific inference. The analytical approach, which establishes relationships within the experimental setting itself, is representative of that type of inference which is characteristic of Mill's Canons of Induction. The interpretative approach, which goes beyond the experimental setting and relates the problem to previously established associations, is representative of that type of inference which characterizes scientific theories of a general nature, as for example some of the theories of nerve action which employ previously acquired information concerning electrical phenomena.

THE TIME-ERROR IN PSYCHOPHYSICAL JUDGMENTS

By CARROLL C. PRATT, Harvard University

If two equal stimuli are presented successively for purposes of comparison with respect to some psychological dimension, more judgments will be secured in the direction of increase (*greater, louder, higher, heavier, longer,* as the case may be) than in the direction of decrease. If several comparison-stimuli are used, a similar preponderance of *greater* judgments will result, provided the comparison stimuli are symmetrically distributed about the standard and the standard is always presented first. In the dimension of intensity this fact, generally referred to, after Fechner, as the negative time-error, was firmly established during the first days of psychophysical experimentation.

Various attempts were made to account for the phenomenon. Müller, working chiefly with lifted weights, was inclined to regard the falling-off in effective comparison-value of the first stimulus as due to fatigue.[1] This view would be plausible enough for intensity of weights, but the time-error appears in other dimensions and modalities and is presumably to be ascribed to some more general factor. In weights fatigue would cause the second member of a pair to appear heavier. In sounds, however, where the time-error is equally marked, fatigue, if it operated at all, would cause the second member to appear softer. Another frequent contention was that the physiological effect of the first stimulus does not completely disappear between the standard and comparison, with the result that an effective increment is added to the second stimulus which favors the judgment in the direction of increase. Borak was able, however, to dispose of this view.[2] Since the physiological effect presumably decreases with the passage of time, a longer interval between standard and comparison should tend to return the judgments more nearly to symmetrical distribution. The exact opposite was found. With increase of time-interval the preponderance of *greater* judgments becomes even more pronounced.

The most frequent explanation of the time-error remained at a more commonsense psychological level. When a judgment is made upon the second stimulus in terms of the first, the standard is already past and gone. The comparison must therefore be based upon some sort of imaginal revival of the first impression. But, if this is true, it is only natural to suppose that the revived image is less intense than was the original impression. The comparison-stimuli are therefore always judged against a lowered standard, so that the *greater* judgments outnumber the *less* and the points of subjective equality uniformly occur below the level of objective equality. The explanation seemed to fit the facts perfectly until Köhler re-

* Accepted for publication February 4, 1933.

[1] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit,* 1899, 117.

[2] J. Borak, Über die Empfindlichkeit für Gewichtsunterschiede bei abnehmender Reizstärke, *Psychol. Forsch.,* 1, 1922, 385 f.

minded psychophysicists that the literature of introspection furnishes abundant evidence that successive comparison-judgments are frequently made without any trace of an image whatsoever.[3]

Köhler therefore preferred to state the matter again in terms of physiology. Borak and others had suggested that the physiological effect or trace of the first stimulus combines with the neural processes set into operation by the incoming second stimulus, thus conferring upon the comparison-impressions an enhancement in the direction of increase. Borak's own experiments indicated that this view was hardly tenable. There is another alternative, however, with respect to the neural trace of the first stimulus. It may not combine with the second stimulus. It may instead retain relative independence and furnish the physiological level *against* which the second stimulus acquires effective value for comparison. Köhler defended this view chiefly because, among other things,[4] it accords well with the increase in the amount of the negative time-error with the lengthening of the time-interval between standard and comparison beyond the initial two or three seconds. If the trace is sinking, and if furthermore the trace is the primary condition for touching off the comparison-judgment when the second stimulus comes along, then the degree of preponderance of *greater* judgments at any point along the temporal line after the three-second interval is easily accounted for. Explanation in terms of a sinking physiological trace does service for a non-existent fading image.

Lauenstein now proposes a modification or enlargement of Köhler's trace-theory.[5] The theory as stated by Köhler is susceptible to two different interpretations. "Wir können erstens annehmen, dass alle Spuren durch den Stoffwechsel allmählich abgebaut werden, und dass darauf der negative Zeitfehler bei längeren Zwischenzeiten zurückzuführen ist. Oder wir können zweitens voraussetzen, dass sich unter den Umständen dieser Versuche benachbarte Spuren auf die Dauer aneinander angleichen. Dann wäre der negative Zeitfehler durch die Annäherung der Spur des ersten Prozesses an die Spur des Zustandes der Reizlosigkeit zu erklären. Zwischen diesen beiden Hypothesen lässt sich nun leicht eine experimentelle Entscheidung herbeiführen. Wenn nämlich die zweite Hypothese richtig ist, so muss sich der Zeitfehler umkehren, wenn ich dem ersten Reiz statt der Reizlosigkeit einen noch 'kräftigeren' Reiz unmittelbar folgen lasse. Die Angleichung an den zugehörigen 'stärkeren' Prozess wird eine 'Verstärkung' der Spur bewirken, und es muss sich ein positiver Zeitfehler statt eines negativen ergeben. Trifft aber die erste Annahme zu, so kann diese Veränderung des Verlaufes keinen Einfluss auf den Zeitfehler haben."[6]

The experimental attack suggested in the preceding quotation was carried out by Lauenstein in the Berlin laboratory, with results which were regarded as inconsistent with Köhler's interpretation of the trace. When the interval between the two stimuli presented for comparison was filled by a stimulus of the same modality considerably stronger than the standard, then, instead of a negative time-error, a posi-

[3] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 115-175.

[4] I am not concerned in the present context with the elaborate physico-chemical speculations with which Köhler fortifies his arguments. They are irrelevant to the present problem.

[5] O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Psychol. Forsch.*, 17, 1932, 130-177.

[6] *Ibid.*, 152.

tive time-error appeared. When a weaker intervening stimulus was used, the usual negative time-error returned. Lauenstein therefore assumed that, since the introduction of a strong background-stimulus produces a preponderance of judgments in the direction of decrease, the physiologically effective trace must be regarded at any one moment during its course not as a mere matter of *sinking*, but rather as a process of *assimilation* going on between the trace and the neural effects of surrounding stimuli.

Unfortunately Lauenstein's own theory is also subject to equivocal interpretation, and must be regarded as too hasty a generalization. Within the very special conditions of his experiment his hypothesis is reasonable enough; but the conditions are very special indeed. If an hypothesis is tenable in one set of circumstances, it is not necessarily upset when a different set of circumstances yields different results. The proposition, if *A* then *B*, does not mean, if *A'* then also *B*. When two objects are presented to a person for comparison with respect to some characteristic, it is not to be expected that the comparison will come off in the same manner no matter on what background the objects are placed. The relative pleasantness of two odors might well be different in a fish-glue factory from what it would be in an otherwise odorless room.

Can it be assumed, moreover, that absence of an intervening stimulus is the limiting case in a descending series of sensory impressions? How is one to conceive "die Spur des Zustandes der Reizlosigkeit?" Does the trace which results from the presence of no stimulus at all also sink with time or does it assimilate itself to its neighbors? If one is to assume, as Lauenstein apparently does, that the trace from stimulus-absence is to be treated in the same manner as the traces from actual present stimuli, then it would follow that the negative time-error from no intervening stimulus must be greater than that from any comparison-judgments in which intervening stimuli are introduced. Assimilation to zero, in other words, must produce a lower trace, and hence a greater preponderance of greater judgments, than assimilation to any value above zero. Evidence can be found, however, which indicates that such is not the case.

The practice of presenting three and even four stimuli in a temporal group for the purpose of studying various types of comparison-judgment has been adopted on several occasions in the Harvard laboratory.[7] The only results comparable to those reported by Lauenstein which at the moment lie ready at hand are presented in Table I. In these experiments the background-stimulus was introduced and then removed between each standard and comparison, whereas Lauenstein used a continuous background. At the level of the results themselves, however, this difference makes no change in the direction of the time-errors. The impressions on which the judgments were made were produced by various heights of fall of a sound-pendulum. The standard stimulus in all cases was set at 45°. The variable stimuli were 37, 41, 45, 49, and 53°, except in one or two cases in Series II and III, when it was necessary to shift them up or down to offset too marked a preponderance of judgments in the direction of decrease or increase. The slight correction necessary to produce a linear series of intensities on a sound-pendulum, $i = \sin^2(\theta/2)$, was not made, inasmuch as an accurate determination of limens was not involved. Fifty judgments on each stimulus in each series were secured,

[7] See, for example, W. D. Turner, Intra-serial effects with lifted weights, this JOURNAL, 43, 1931, 1-25.

making therefore 2250 judgments in all. The data were treated by Urban's phi-gamma process of calculation.

Preliminary experiments made it clear that the time-error became negative for all three *O*s at about a three-second interval between standard and comparison. The pause between these two stimuli was therefore kept constant at 4 sec. for all judgments. In Series I (Normal) no stimulus was interposed between standard and comparison. The background was phenomenally empty. In Series II (Loud) an intermediate stimulus, produced by a fall of the pendulum from 70°, was inserted between the standard and comparison. In Series III (Soft) a fall of the pendulum from 20° was interposed.

It is clear from an inspection of the table that the results are similar to those secured by Lauenstein. The table presents, for each series, the upper and lower limens in columns 1 and 2, the interval of uncertainty in column 3, and the point of subjective equality in column 4. In Series II (Loud), in which a strong intermediate stimulus comes onto the background, the PSE for each observer is above

TABLE I

UPPER AND LOWER LIMENS, INTERVALS OF UNCERTAINTY, AND POINTS OF SUBJECTIVE EQUALITY FOR THREE SERIES OF JUDGMENTS ON AUDITORY INTENSITIES

	I (Normal)				II (Loud)				III (Soft)			
	U	L	Un	PSE	U	L	Un	PSE	U	L	Un	PSE
G	46.79	42.53	4.26	44.66	50.25	42.73	7.52	46.49	46.59	39.00	7.59	42.79
H	45.38	41.68	3.70	43.53	52.80	45.19	7.61	48.99	44.80	38.57	6.23	41.68
F	46.28	42.17	4.11	44.22	55.54	46.43	9.11	50.98	44.74	39.46	5.28	42.10

45°, indicating therefore a *positive* time-error. The trace of the standard stimulus, according to Lauenstein, assimilates to the very much louder sound in the background and hence produces a preponderance of judgments in the direction of decrease. In Series III (Soft) the reverse situation obtains. The trace assimilates to a very much softer sound, so that judgments in the direction of increase predominate. So far the results agree perfectly with Lauenstein's hypothesis. But how about results made upon a silent background? In Series I (Normal) the trace must assimilate to a still softer sound than in Series III, namely, to none at all (*Reizlosigkeit*). The PSE should therefore be lower in I than in III. For each *O*, however, the PSE in I is higher than the PSE in III. This fact consequently makes it questionable whether Lauenstein is justified in extending his hypothesis to include the normal comparison-judgment in which the background is phenomenally empty.

The size of the intervals of uncertainty is also worth noting. With the possible exception of *F* in Series III, these values are all considerably larger in Series II and III than they are in Series I, indicating that, although the intermediate stimuli are not included within the *Aufgabe* of comparison, they nevertheless obtrude themselves as very real phenomenal distractors. It is not at all unlikely, as a matter of fact, that this very distortion of the normal comparison-judgment represents the basic difference between Lauenstein's results and the results usually secured in psychophysical judgments.

Similar findings with lifted weights have been reported by Guilford and Park.[8]

[8] J. P. Guilford and D. G. Park, The effect of interpolated weights upon comparative judgments, this JOURNAL, 43, 1931, 589-599.

When weights considerably heavier or lighter than the standard were interpolated, the limens, with one exception, were all larger. In the normal series a third weight was also interpolated, but it had the same value as the standard and presumably acted therefore only as a very slight distraction. The PSE of the normal series were also, with one exception, higher than those from judgments in which a light weight was interpolated, but since the normal series had an interpolated weight as well, this difference in amount of negative time-error is inconclusive with respect to the question of assimilation.

Since the course of the time-error with the passage of time and with different time-intervals is anything but a simple function, the results in Table I must not, of course, be regarded as final. They are merely suggestive and are reported as evidence that the issue between *assimilation* and *sinking* is still open. Since the amount of the time-error varies with the number of preceding judgments, a few judgments with

TABLE II

NUMBER OF JUDGMENTS OF THREE CATEGORIES IN TWO SERIES OF EXPERIMENTS WITH LIFTED WEIGHTS

D% = difference in percent between judgments of *greater* and of *less*.

Series	Stimulus (grm.)	B			D%	M			D%	C			D%
		g	e	l		g	e	l		g	e	l	
I (Light Weight)	108	8	1	1		9	1	0		8	2	0	
	100	5	3	2	−33.4	5	3	2	−43.4	6	4	0	−40.0
	92	2	6	2		4	3	3		2	4	4	
II (Normal)	108	8	1	1		9	1	0		9	1	0	
	100	2	4	4	−6.6	4	5	1	−23.4	5	3	2	−20.0
	92	1	5	4		2	1	7		0	4	6	

lifted weights were taken in order to see what the outcome would be before the effect of accumulated judgments could be serious. The real issue centers about the relative sizes of the negative time-error when there is a very weak interpolated stimulus as contrasted with judgments in which no intermediate stimulus is introduced. It was therefore unnecessary to run through a series with a heavier interpolated weight.

These results are presented in Table II. The standard weight was 100 grm., the variable weights, 92, 100, and 108. In Series I the intermediate light weight was 15 grm. In Series II no intermediate weight was used. The interval between standard and comparison, for both series, was 4 sec. The number of *heavier, equal,* and *lighter* judgments is given under the columns *g, e,* and *l.* D% refers to the difference, in percentage, between the *greater* and *less* judgments and is used by Lauenstein as a measure of the size of the time-error. According to Lauenstein the size of D% should be greater in Series II than in Series I. It will be noted that the reverse is the case.

The conclusion which these results suggest is obvious. When the stimulus which sets off a comparison-judgment comes after an intermediate stimulus or a background considerably greater than the standard, then the preponderance of judgments in the direction of decrease makes it reasonable to argue that the trace of the standard

has, by means of assimilation, moved upward. When the background or intermediate stimulus is considerably less than the standard, the process of assimilation is reversed, thus producing preponderance in the direction of increase. When, however, there is no intermediate stimulus nor appreciable background, the trace merely *sinks*. With the exception of vision, this last case is the normal situation for comparison-judgments in psychophysics. Whatever of assimilation exists is brought about either by intra-serial effects[9] or by a general level or equilibrium of judgment[10] created by preceding comparisons either near or remote in time.

Lauenstein insists that in nearly all judgments there is some sort of effective background. "In fast jedem Sukzessivvergleich ist dieser Fall verwirklicht, da in der Pause zwischen den Wahrnehmungen 1 und 2 gewöhnlich irgendwelche anderen Wahrnehmungen liegen."[11] A bit later, however, he seems less certain, and suggests that only a limited number of items can possibly be effective for assimilation. "Die Angleichung der Spuren an ihre Umgebung gilt deshalb nur für ganz bestimmte einfache Verhältnisse. Auf jeden Fall werden sich überhaupt nur die Spuren aneinander angleichen können, die in einem konkreten Strukturzusammenhang miteinander stehen, und man kann wohl erwarten, dass die Angleichung um so stärker sein wird, je enger dieser Zusammenhang ist."[12] The latter suggestion seems by far the more reasonable one. Certainly in judgments of sounds and weights "irgendwelche anderen Wahrnehmungen" do not generally clutter the perceptual field. For sounds the background is silence, and silence seems not to stand in a close enough structural connection with the auditory impressions to serve as a source of assimilation.

Since the earliest days of psychophysical work the preponderance of *greater* judgments in the normal situation of comparison has been ascribed to the falling-off or sinking, with the lapse of time, of the effective value of the first stimulus. What the exact mechanism of this depreciation is, no one knows. Köhler seeks to lend it clarity by placing it within the framework of physico-chemical dynamics. Lauenstein's attempt to replace Köhler's theory is effective, so far, only for judgments made upon artificially induced backgrounds and possibly for vision. He has not yet demonstrated that phenomenally empty backgrounds set up neural processes which combine with the trace of the standard stimulus.

The psychophysical generalization which at present most nearly fits the principal data considered in the preceding discussion can be stated in the following manner. *The effective comparison-value of the first member of a pair of stimuli presented for judgment with respect to intensity decreases in magnitude after the first three seconds, unless the impression from some secondary stimulus constitutes an integral part of the comparison-field, in which case the comparison-value of the first stimulus becomes a function both of time and of the magnitude of the secondary stimulus relative to the first comparison-stimulus.* Experiments are now in progress which are designed to throw more light on certain other factors which influence the course of the time-error.

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[9] S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 126-150; Turner, *op. cit.*

[10] H. L. Hollingworth, The central tendency of judgment, *J. Philos.*, 7, 1910, 461-468; J. G. Beebe-Center, *The Psychology of Pleasantness and Unpleasantness*, 1932, 222-237.

[11] *Op. cit.*, 135.

[12] *Ibid.*, 170.

AESTHETIC PROPORTION

By FRANK C. DAVIS, University of California at Los Angeles

Students in the sections of the course in Elementary General Psychology at the University of California were asked to make an attempt to visualize what seemed to them, at the moment, the rectangle of most pleasing proportions, and then to draw it freehand on blanks provided them.[1] About 40 min. later they were asked once more to visualize what seemed to them at *that* moment the most pleasing rectangle, and to draw it freehand as before. The blanks were 5 in. square. The *S*s were requested to draw the base of the rectangle longer than the height, except in case they chose the square as being "most pleasing" in its proportions.

The *E* wished to determine (1) what the general reaction of these *S*s would be to such an *Aufgabe;* in case there were fairly widespread acceptance of the task, he wanted to find out (2) whether those who claimed to be lacking in visual imagery were reliably differentiated (with respect either to deviations from a central tendency, or with respect to the degree of consistency of their two preferences) from those who claimed to be able to visualize clearly their most preferred rectangle or rectangles.

Concerning (1) it may be said that philosophers sometimes comment caustically upon the attempts of Fechner to lead the way in the psychologizing of aesthetics, with aesthetic proportionality his starting-point. It should be patent to all, they argue, that proportionality is always *of something,* and hence there can be no "most pleasing proportions" *per se.* This they hold in spite of the fact that most people will confess to having a favorite color, or a favorite odor, whereas those individuals who are (relatively) musically unsophisticated may even claim that they like a pitch of specified vibration rate, or a given musical interval, better than other pitches and intervals. At any rate, the one way to find out whether people find proportionality pleasing in its own right is to put the question to experimental test, as Fechner did. If it be found that they can and do express such a preference, then the question is not psychologically an idle one as to the source or sources of the preference.

Those psychologists who stress the rôle played by the environment in determining our responses insist that preference for this or that proportion, for this or that rhythm, or for any other datum whatsoever, is primarily a matter of habituation. "We like what we are used to," is the simplest statement of this formula.[2] The

* Accepted for publication November 18, 1931.

[1] The data referred to in this note were collected at the University of California at Berkeley as part of an investigation dealing with the functional significance of individual differences in imagery.

[2] White and Landis believe that they have good grounds for the conclusion that "with one type of material (the human profile) and with one type of observers (children and college students), there is a tendency for familiarity to produce pleasure and for unfamiliarity to produce displeasure or dislike." R. K. White and C. Landis, A reply to Dr. Murray's concept of aesthetics, this JOURNAL, 43, 1931, 290.

proportions with which we are most familiar, the ones to which we have been most often exposed, will *ipso facto,* according to this view, be the proportions which we prefer. A preference for green or blue is readily accounted for on such a basis, since these are the colors most prevalent in 'nature'—the colors of grass and trees, of sky, lake, and ocean; and so on.

There are many persons, however, to whom this manner of explaining our preferences seems very definitely to be an oversimplification of the facts. In the matter of aesthetic proportion, for example, it would seem that if the factors of ubiquity and propinquity were the only or chief ones operating to determine preference, then the Ford Model-T motor vehicle should have been esteemed an aesthetic gem, since it undeniably outnumbered every other make of automobile on the highways for a span of years longer than one now cares to contemplate. But as a matter of fact, the ugliness of its proportions had as unanimous agreement as did its utility and its stamina.

The Gestaltists, of course, will seek a different explanation of preference. While presumably not denying that something like 'habituation,' or 'conditioning' in the relatively loose, less sharply defined sense in which Watson often uses it, plays a certain part in behavior-phenomena, they will characteristically place much more emphasis upon the rôle played by the organism itself in the organization of its responses. For Watson and other behaviorists of the "end-term" variety the nervous system "conducts;" for the Gestaltists it also "configures," and for them this configuring property is the *sine qua non* in the explanation of psychological phenomena.

During the course of the present experiment only 4 out of a total of 310 *S*s refused outright to express a preference. These four, it developed, had been forewarned in a class in the philosophy of aesthetics, in which they were then enrolled, that they might expect to be asked by experimental psychologists to do just such an illogical thing, and they were primed with all the possible arguments against such a procedure. When, however, the *E* drew on the blackboard two rectangles, one 2 ft. long by 2 in. wide, the other much shorter and somewhat wider, all four agreed that for some reason the latter seemed more pleasing than the former.

Instead of averaging the two ratios which each *S* yielded, on the assumption that the 'most pleasing rectangle' would necessarily remain stable for most *S*s over a 40-min. interval, with the differences in the rectangles-as-measured due either to inaccuracies in the drawing or in the measuring, or both together, the two were treated as independent preferences, on the assumption that a given *S* might very well have several, or many, approximately equal preferences, one of which would come out on the first occasion and another on the second. It was found that the ratios (base over height) of the most pleasing rectangles drawn by these *S*s ranged from 1.00 (the square) to 9.75, with a highest mode at 2.00, but with other modes, almost equally high, at 1.75 and at 2.25. See Fig. 1. It is interesting to note that in the interval from 1.55 to 1.64, which includes the so-called 'golden section,' slightly less than 3% of the total number of preferred proportions are found. Between the limits, 1.60 to 2.29, are included 54% of the 620 preferences.

Fig. 1 gives the sort of picture which one might expect if the experimental group were markedly heterogeneous with respect to whatever the factors may be which determine "aesthetic preferences" as recorded under the conditions of this experiment. If these *S*s differed considerably with respect to (1) their interest in

the task, (2) their conscientiousness in attempting to follow directions, (3) their ability to visualize a "most preferred" rectangle, (4) their ability to draw freehand the rectangles which they "imagined," and (5) the proportion or proportions preferred by them, then the distribution to be looked for would be something like the one found. The task itself might be expected to interest *Ss* of more or less "general" aesthetic sensibility (if such general sensibility be for the moment hypothetized) to a greater extent than it would *Ss* of predominantly scientific or legalistic turns of mind, for example. If some *Ss* were aware of no clearly defined preferences, but

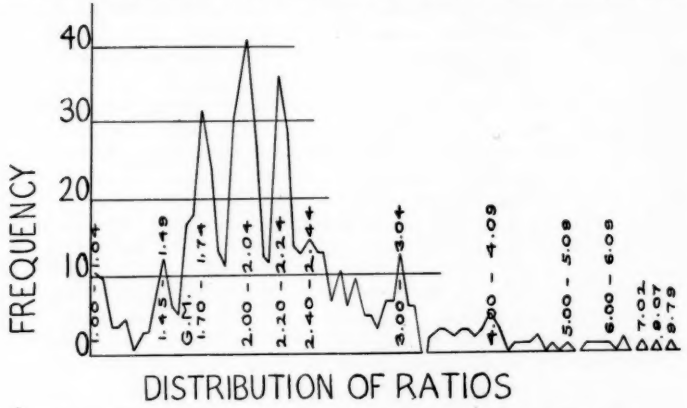

FIG. 1. DISTRIBUTION OF RATIOS OF THE DIMENSIONS OF MOST PREFERRED RECTANGLES

(310 *Ss*, 2 preferences each, 620 ratios. The ratios are distributed in class intervals of 0.05 to the point where the abscissa is first broken. Beyond that point they are grouped in class intervals of 0.10, for convenience of presentation.)

coöperated nevertheless to the extent of drawing their two rectangles in more or less haphazard fashion, their rectangles would tend to be random in their proportions, and also with respect to the degree of similarity which the second bore to the first. But if *all* the *Ss* had approached the task with this attitude, the chances would seem to be remote (1) that any pronounced central tendency would appear, (2) that any appreciable number of *Ss* would duplicate the proportions of their first rectangle in another drawing 40 min. later.

As a matter of fact, some 31% of the paired preferred-ratios differed by less than 0.10, indicating for the *Ss* concerned a high degree of consistency. Two out of every three of these 'consistent' preferences were for one or another of the modal ratios, 1.75, 2.00, or 2.25. In the case of some of the *Ss* in this group, the absolute dimensions were three or more times as large in the case of one of the rectangles as they were in the other, with the two ratios nevertheless identical. The total range of the ratio-differences was from 0.00 to 3.21, the greatest frequency being that just noted of 31% for differences between 0.00 and 0.10; the next highest frequency for any one class interval was 17% for differences between 0.20 and 0.30. However, ratio-differences from 1.00 to 3.21, although scattering, totalled 20% of

the entire number. This means that while nearly a third of the *Ss* in this experiment have so well in mind some particular most-preferred rectangle that their two drawings, though separated 40 min. in time, approach identity, nevertheless one out of every five of the total number of *Ss* manifests very unstable preferences indeed under these same experimental conditions. A coefficient of correlation, under the circumstances, may be a somewhat ambiguous index to the degree of 'reliability,' or better, consistency, of such preferences. It was computed, however, and proved to be 0.67.

Whatever factors may be at the root of the individual differences noted, imagery is not one of them. The writer's 'hunch,' that those reporting weak visual images or none at all would be found at the extremes of the distribution, was unsupported by the experimental results. 'Intelligence,' also, as measured by Army Alpha test, proved not to go with a tendency either toward consistency of preferences, under such conditions, or toward variability, as is indicated by a correlation coefficient of $-0.23 \pm .06$ between size of the ratio-difference and Alpha score.

These results are of interest in two connections. First, the wide range of the rectangle-ratios "most preferred," and the pronounced modes, lend emphasis once again to the conclusions reached by Angier,[3] that it is fallacious to look for a "golden section" in the average of the preferences of a number of individuals. Furthermore, since so many *Ss* express a preference for one proportion on one occasion, and then register a preference for a markedly different proportion on another occasion, it seems obvious that the psycho-physical method of average error would yield ambiguous results with such *Ss* unless intelligently applied. If, for example, the *S* were one who happened to like rectangles of the proportions of 3:1, 2.25:1, and 1.62:1 equally well, and if his 'out' determinations started well beyond the 3:1 proportion while his 'in' determinations started well inside the 1.62:1 proportion, he would presumably get a series of 'out' determinations fluctuating around the 3:1 ratio and a series of 'in' determinations fluctuating around the 1.62:1 ratio, and to take the mean of the means of the two series as indicating his truly '*most*-preferred' proportion would be an unintelligent procedure of which perhaps no competent experimentalist would be guilty.

The above line of reasoning is of course contingent upon (1) the distribution of preferences being genuinely multi-modal, and (2) the existence of *Ss* who can be accurately described as 'balanced' in their proportional preferences between several distinct ratios. There is, naturally, no *a priori* reason to expect a distribution of preferred-proportions to be unimodal and 'normal' in shape, although it may be admitted that that was the writer's preëxperimental bias. (On the other hand, it is possible, though it seems hardly likely, that the curve shown in Fig. 1 would eventually smooth out, with the addition of many more cases, with a resultant mean probably somewhere between 1.70 and 2.29.)

The second connection in which the present data are of interest has to do with the results reported by Hambidge[4] from his measurements of Greek vases and temples. Hambidge reports the prevalence in ancient Greek art and architecture

[3] R. P. Angier, The aesthetics of unequal division, *Psychol. Monog.*, 4, 1905, (no. 17), 541-561.

[4] J. Hambidge, *The Parthenon and other Greek Temples: Their Dynamic Symmetry*, 1924.

of "root-ratios." Rectangles having these ratios have what he has termed "dynamic symmetry"—the total surface of such a rectangle is divisible into a number of exactly similar rectangles. Especially prevalent in Greek works of art are the ratios 1.72 + $(3)^{\frac{1}{2}}$, 2.00 $(4)^{\frac{1}{2}}$, and 2.24 — $(5)^{\frac{1}{2}}$. These approximate the modal ratios around which cluster the majority of the preferences indicated by the *S*s in the present experiment.

The interpretation of these results is far from obvious. Hambidge's "dynamic symmetry" has not been favorably received by those philosophical aestheticians who resent both the application of measurement and the attempt to educe general principles in the field of aesthetic preference. But, while the theory may perhaps be 'laughed off,' the measurements cannot be, and the accuracy of Hambidge's measurements seems not to have been challenged.

To the writer's knowledge, Langfeld is the only psychologist who has taken note of Hambidge's data and of his conclusions. He notes with apparent approval the latter's emphasis upon areal, rather than linear, symmetries.[5] From the standpoint of the *Gestalt* psychology Hambidge's findings, and the results from the present experiment, might be taken to indicate that for some reason (at present obscure) rectangles having approximately these proportions effect better 'closure' for most *S*s than do other rectangles. On this hypothesis the basis for the preference would presumably be the resolution of tension which accompanies, or which is, closure. The tensions, in this instance, might be peripheral, cortical, or both.

Summary

(1) Contrary to the *a priori* judgment of certain philosophically-minded aestheticians, college students in general find it neither absurd nor impossible to imagine a "most-preferred" rectangle.

(2) When the base-to-height ratios of 620 rectangles (2 each from 310 *S*s) were computed and tabulated, the resulting distribution showed the preferences to be grouped around several modal ratios.

(3) College students differ greatly with respect to the stability of their preferences under the given experimental conditions (freehand drawing of two rectangles, 40 min. apart).

(4) The facts presented in the results of this and former experiments indicate the fallaciousness of attempts to establish *a* 'golden section' based upon averages, either of the preferences of a group of *S*s or of a number of preferences from a single *S*.

(5) No relation was found between the imagery reported and the *S*'s performance in the drawing of preferred rectangles.

(6) No significant relationship was found between 'consistency of preference,' under the conditions of this experiment, and Alpha scores.

(7) The modal ratios for the most preferred rectangle in the case of the present investigation approximate closely the ratios reported by Hambidge as those characteristically employed by the Greeks in their planning of the proportions of temples and vases.

(8) The *Gestalt* principle of 'closure' seems to supply a possible psychological basis for aesthetic preferences of this sort.

[5] H. Langfeld, *The Aesthetic Attitude*, 1920, 129.

IMPROVED NORMS FOR FOUR PERSONALITY TESTS

By Ross Stagner, University of Wisconsin

While psychiatric analysis of personality in terms of individual interviews is no doubt the most thorough and accurate method of study available at the present time, the cost in both time and money makes its use prohibitive. An institution desiring to make a survey of its students in developing a mental hygiene program would find such a method quite impracticable. The only alternative offered by psychology at the moment is the use of group tests, which more than compensate in utility for their loss in validity.[1]

A number of useful group tests are available for the college level. These have been developed on the basis of the best knowledge available with reference to the trait tested; they have fairly high reliabilities (high enough for group testing) and some measure of validity. One fact tends to reduce their utility, and this is the lack of adequate norms.

The individual developing a test usually does so during his tenure at a single educational institution. For normal groups he naturally uses the students available there. As we propose to show, the personality traits average in a given institution may deviate considerably from those prevalent elsewhere, and consequently the testing of a small group at another institution may lead to quite erroneous conclusions concerning the relative quality of the students, since the standard group does not represent a true standard.

The best method for the development of nation-wide norms for a given test would be that of a cumulative frequency distribution, based on the administration of the test to students in colleges taken at random over the entire country. This method works admirably with the American Council Psychological Examination.

Another method, less desirable, is the determination of the average score and standard deviation for a large number of groups. This does not offer the possibilities of interpretation which one has with the frequency distribution, but it insures a stable average and a known degree of variability. Garvey[2] has recently published a formula which makes it easy for anyone using a calculating machine to determine the standard deviation for a large group from the means, sigmas and frequencies of the component groups.[3] This formula has been employed in the present case to determine the 'true' means and standard deviations from the studies reported in the literature.

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* Accepted for publication November 5, 1931.

[1] It should be understood that we refer to their use in surveys, selecting individuals who require further analysis by the interview method.

[2] C. R. Garvey, Sigmas of combined distribution calculated from sigmas, means and frequencies of component distributions, *J. Educ. Psychol.*, 22, 1931, 307-310.

[3] The formula is: $\sigma^2_{12} = [[(M^2_1 + \sigma^2_1)\ N_1 + (M^2_2 + \sigma^2_2)\ N_2] / N_1 + N_2)] - M^2_{12}$. It may, of course, be extended to any desired length.

In the course of administering various personality tests, we have come to feel the need for norms which would not be influenced by the type of students common to a single university. The task of collecting them, however, has been rendered difficult by the practice, common in articles on testing, of presenting correlations or other data without the measures of central tendency and variability on which they are based. Frequency distributions are even more rare. The expense of publishing tables is causally associated with this situation. Of the large number of articles using the Pressey X-O test, for example, we have not found one giving a frequency distribution of scores, and relatively few giving means and sigmas.

TABLE I

IMPROVED NORMS OF CENTRAL TENDENCY AND VARIABILITY FOR FOUR WELL-KNOWN PERSONALITY TESTS

Test	N	M	S.D.
Laird Inventory C 2	1021	15.96	5.03
Social Intelligence (Form 2)	1235	99.18	17.79
Thurstone Neurotic Inventory	1257	43.63	23.67
Pressey X-O Affectivity (Form A)	340	189.24	55.71

We have attempted to derive improved norms for four well known and widely used personality tests: the Colgate inventory C 2 for introversion; the Thurstone neurotic inventory; the Pressey X-O test (Form A);[4] and the Moss Social Intelligence test, Form 2. For the first two, we present frequency distributions from different studies; for the latter two, we have the frequencies only for tests immediately available to us, as we could find no others published. We give means and sigmas based on a large number of cases, for each of the four. Studies on the Allport A-S test offer a large number of cases, but none of the studies are in proper form to be used for norms. The Allport test is separately standardized for men and women, and the same score on a different form has quite a changed

TABLE II

FREQUENCY DISTRIBUTION ON THE LAIRD INVENTORY C2

Score	Frequency		
	Men	Women	Total
37–39	0	1	1
34–36	0	1	1
31–33	1	3	4
28–30	1	6	7
25–27	5	27	32
22–24	10	42	52
19–21	17	64	81
16–18	33	97	130
13–15	41	82	123
10–12	28	66	94
7– 9	6	24	30
4– 6	4	7	11
Total	146	420	566

[4] Only affectivity scores have been compiled, as the idiosyncrasy score should properly be developed anew for each group studied.

TABLE III

FREQUENCY DISTRUBUTION ON THE THURSTONE NEUROTIC INVENTORY

Score	Frequency		
	Men	Women	Total
150–164	0	1	1
135–149	0	0	0
120–134	0	0	0
105–119	5	12	17
90–104	14	13	27
75– 89	32	28	60
60– 74	64	69	133
45– 59	125	120	245
30– 44	177	106	283
15– 29	198	107	305
0– 14	105	33	138
Total	720	489	1209

TABLE IV

FREQUENCY DISTRIBUTION FOR THE PRESSEY X-O TEST

Score	Frequency		
	Men	Women	Total
310–329	0	1	1
290–309	1	0	1
270–289	0	0	0
250–269	0	3	3
230–249	4	8	12
210–229	11	8	19
190–209	6	12	18
170–189	6	12	18
150–169	14	6	20
130–149	13	10	23
110–129	10	8	18
90–109	9	4	13
70– 89	2	3	5
50– 69	1	1	2
30– 49	1	0	1
Total	78	76	154

TABLE V

FREQUENCY DISTRIBUTION FOR THE NEYMANN-KOHLSTEDT TEST

Score	Frequency		
	Men	Women	Total
+36–+42	2	0	2
+29–+35	1	1	2
+22–+28	3	4	7
+15–+21	2	8	10
+ 8–+14	20	11	31
+ 1–+ 7	25	24	49
− 6– 0	18	22	40
−13–− 7	7	10	17
−20–−14	2	9	11
−27–−21	0	3	3
Total	80	92	172

significance. Yet several studies report the scores for the two combined, as though this were a perfectly legitimate procedure. Until this condition is improved, we cannot collect norms.

We are also presenting a frequency distribution for the Neymann-Kohlstedt extroversion-introversion test, which has been published without statistical data. The measures of central tendency and variability on our group for this test are published elsewhere.[5]

Sources. As a matter of perhaps considerable importance to anyone using these norms, we wish to specify the source of each item. By tables, they are as follows:

Table I. Test C 2: Broom,[6] San Diego Teachers College; Whitman,[7] Skidmore College, Colgate University; Stagner,[8] Gustavus Adolphus College.

Social Intelligence test: Broom,[9] Garrett and Kellogg,[10] Columbia; Jersild,[11] Barnard; Stagner.[12]

Thurstone test: Allport,[13] Dartmouth; Barrett, Jacobs and White,[14] Wisconsin; Mears,[15] Wisconsin; Stagner; [16] Thurstone and Thurstone,[17] Chicago.

Pressey test: McGeoch and Whitely,[18] Washington University; Stagner,[19] Wissonsin.

Table II. Broom,[20] and Stagner.[21]

Table III. Allport,[22] Barrett, Jacobs and White,[23] Mears,[24] Stagner,[25] Thurstone and Thurstone.[26]

Table IV. Stagner.[27]

Table V. Stagner.[28]

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[5] R. Stagner. Intercorrelations of some standardized personality tests, *J. Appl. Psychol.*, 16, 1932, 453-464.

[6] M. E. Broom, Critical study of a test of introversion-extroversion traits, *J. Juven. Res.*, 13, 1929, 104-23.

[7] R. H. Whitman, Sex and age differences in introversion-extroversion, *J. Abn. & Soc. Psychol.*, 24, 1929, 207-11.

[8] Stagner, *op. cit.*

[9] M. E. Broom. Note on the validity of a test of social intelligence, *J. Appl. Psychol.*, 12, 1928, 426-28; Further study of the validity of a test of social intelligence, *J. Educ. Res.*, 22, 1930, 403-05.

[10] H. E. Garrett and W. N. Kellogg, Relation of physical constitution to general intelligence, social intelligence and emotional stability, *J. Exper. Psychol.*, 11, 1928, 113-29.

[11] A. T. Jersild, A study of personality, *J. Abn. & Soc. Psychol.*, 25, 1930, 115-20.

[12] Stagner, *op. cit.*

[13] G. W. Allport, Neurotic personality and traits of self-expression, *J. Soc. Psychol.*, 1, 1930, 524-27.

[14] D. Barrett, L. B. Jacobs, and E. M. White, Correlations between four personality tests and certain physical measurements, Thesis, University of Wisconsin, 1931.

[15] C. E. Mears, Interrelationships between academic success, intelligence and social adjustment, M.A. Thesis, University of Wisconsin, 1931.

[16] Stagner, *op. cit.*

[17] L. L. Thurstone and T. G. Thurstone, A neurotic inventory, *J. Soc. Psychol.*, 1, 1930, 3-30.

[18] J. A. McGeoch and P. L. Whitely, Reliability of the Pressey X-O tests for investigating the emotions, *J. Genet. Psychol.*, 34, 1927, 255-70.

[19] Stagner, Emotional factors influencing retention, M.A. Thesis, University of Wisconsin, 1930.

[20] M. E. Broom, *op. cit., J. Juven. Res.*, 13, 1929, 104-23. [21] Stagner, *op. cit.*

[22] G. W. Allport, *op. cit.* [23] D. Barrett, L. B. Jacobs, and E. M. White, *op. cit.*

[24] C. E. Mears, *op. cit.* [25] Stagner, *op. cit.*

[26] L. L. Thurstone and T. G. Thurstone, *op. cit.*

[27] Stagner, *op. cit.*

[28] Stagner, *op. cit.*, Thesis, University of Wisconsin, 1930.

Utility value. The value of the frequency distributions for the Thurstone and Laird tests needs no discussion. These contain a sufficient number of cases to render interpretation of individual scores fairly dependable, and they are based on groups from different college environments, thus eliminating the constant error due to this factor. The frequencies for Social Intelligence, X-O and Neymann-Kohlstedt are based on results from a single group, with too small a number of cases, and can be used only for approximations.

The value of the measures of central tendency and variability presented lies chiefly in the determination of the typicality of groups. (1) Groups from different colleges may be compared with respect to a given trait, and the statistical reliability of the deviations from these norms computed accurately. (2) Groups within an institution may be compared with the norms more accurately than with each other, if the number of cases in each group is small. (3) Extreme groups selected for special study may be picked from cases scoring plus or minus one standard deviation from the mean, using these values, which are more likely to be dependable than those determined from a smaller sample.

As an illustration of the usefulness of such norms, we may analyze the results of the Gustavus Adolphus tests, since this is the only group taking as many as three of the four tests. The mean for this group on the C 2 test was 15.95, and we may therefore feel quite certain that this group is normal in respect to introversion, as the norm value is 15.96.

The mean for this group on the Social Intelligence test is 96.16, and is the lowest group mean reported. It is 3.02 points below the mean norm, but this difference is less than three times its standard error. The difference is about 70% of what it should be to insure a statistically true difference.

When we consider the results for the Thurstone test, we find that this group has an average score of 51.95. This is 8.32 points more than the norm, and is the highest group mean reported. It is more than three times the standard error of the difference, and we may legitimately conclude that there is a true difference present, that is, that this group is actually more neurotic than the average college group, in so far as this test is a valid measure of the neurotic temperament. This analysis also shows that while introversion and neurotic traits are positively correlated, in a group it is quite possible to find an extreme amount of one and perfect normality of the other.

It is to be hoped that individuals using these personality tests will add to the number of cases represented in these norms, by combining their data with those here published. This is especially needed in the case of frequency distributions, as these are necessary for the adequate interpretation of the score of a single individual.

SUMMARY

(1) Means and sigmas for four standard personality tests have been calculated from the data of other investigators and the author.

(2) Frequency distributions have been combined to give more valuable and useful interpretative devices.

(3) The utility of these norms is discussed and illustrated.

THE INFLUENCE OF A SOCIAL FACTOR UPON THE APPRECIATION OF HUMOR

By RUTH EASTWOOD PERL, Columbia University

The specific problem under consideration in the research here reported is whether jokes presented under certain social conditions are judged to be more or less funny than equally humorous jokes presented under different social conditions; the investigation tries to obtain some quantitative measure of this difference.

Allport says that "social environment is necessary in the rôle of a contributory as well as a direct stimulus for our mirthful response."[1] He feels that there are three causes which underlie the hilarity of the crowd; (1) the large number of facilitating stimuli present, such as the sight and sound of others laughing; (2) the release of common restrained impulses; and (3) the feeling of moral sanction for their release.

H. T. Moore performed an experiment slightly similar to the one about to be discussed but using other material.[2] His experiment consisted of measuring a suggestive influence, such as expert opinion, or group judgment, in terms of the number of reversals of individual judgment occasioned by it as compared with the number that might have been expected by chance. He worked with three types of material; namely, linguistic usage, ethical judgments, and musical judgments.

Wheeler and Jordan made an experiment along these same lines.[3] They used questions on economic issues, politics, and campus affairs. They counted the reversals of opinion which took place before and after group opinion was made known to the subjects, or rather, scored the percentages of opportunities to change that each individual accepted.

Preliminary experiments. In the present experiment, the first task was to compile three equated lists of jokes. To do this, a preliminary experiment was performed using as subjects 19 women undergraduates who were members of the class in Experimental Psychology at Barnard College and 12 men undergraduates of the class in Experimental Psychology at Columbia University. Every *S* was given a mimeographed copy of a list of 160 jokes, divided into 4 sets of 40 each. These jokes were as varied as possible. They were gathered from American and English humorous magazines, joke books, vaudeville acts, and by asking people for the best joke they could think of at the moment. With the lists of jokes, the *S*s were given directions as follows.

You are being handed herewith 4 sets of about 40 jokes each. Kindly work on them in absolute privacy, and do not discuss your results until you have handed

* Accepted for publication November 16, 1931.

[1] F. H. Allport, *Social Psychology,* 1924, 257 f.

[2] H. T. Moore, The comparative influence of majority and expert opinion, this JOURNAL, 32, 1921, 16-20.

[3] D. Wheeler and H. Jordan, Change of individual opinion to accord with group opinion. *J. Abn. Psychol. & Soc. Psychol.,* 24, 1929, 203-207.

them in. You are to work on one of the 4 sets each evening for 4 evenings (so that you do them all).

We are asking you to read the jokes over to yourselves one at a time and grade them. Be sure to grade each joke as you read it, and do not work back over them. You are to grade the jokes on a 5-point scale: A = excellent or most funny, or one of the 20 best jokes out of 100 that you have ever heard; B = good or very funny; C = fairly funny; D = poor or just slightly funny; and E = very bad, not at all funny or equal to one of the worst 20 jokes out of a 100 that you have ever heard.

From these results three lists of equated jokes were compiled in the following manner. First, all the jokes the judgments of which did not show a strong central tendency, were discarded. If 4/5 of the judgments of a joke did not fall within one grade each side of the median, the joke was not used. For example, if the median of the judgments of joke No. 1 was B, then 4/5 of all the *Ss'* grades would have to be A, B, or C for the reactions to the joke to be considered consistent. Also, if one vote more or less would shift the median so that the joke would not pass the 4/5 test, it was discarded. The use of this criterion left 75 jokes which were given quantitative scores by multiplying the frequency of a joke's A votes by 4, its B by 3, its C by 2, its D by 1, and its E by zero, and then adding these together. If, for example, the judgments of joke No. 4 were arranged as follows, 6 A, 13 B, 8 C, 2 D, and 2 E, then it numerical score would be 6 x 4, plus 13 x 3, plus 8 x 2, plus 2 x 1, plus 2 x 0, which equals 81. From these scores the jokes were arranged in an order of merit and finally three sta-

TABLE I

SHOWING AVERAGE RATING OF EVERY JOKE ON THE THREE LISTS

Joke No.	List		
	A	*B*	*C*
1	1.7	1.8	2.0
2	0.6	0.7	2.1
3	1.3	0.8	0.6
4	1.4	1.1	1.5
5	1.9	1.7	2.2
6	1.8	1.7	2.9
7	1.1	3.0	1.9
8	2.1	1.6	1.4
9	1.5	1.3	2.4
10	1.9	1.6	1.3
11	1.9	1.3	1.7
12	1.9	2.2	2.6
13	1.9	1.8	2.3
14	1.6	1.8	1.6
15	1.8	0.9	1.7
16	1.8	2.6	2.2
17	1.6	2.9	1.9
18	1.3	1.7	1.2
19	0.5	0.6	1.2
20	2.0	2.2	3.2
21	1.3	1.3	2.8
22	1.8	1.1	2.2
23	1.3	1.4	1.3
24	2.1	2.7	2.6
25	2.0	1.8	1.7

tistically equated lists of 25 jokes each were made. The jokes in one of the three lists were arranged in random order and the other two lists were matched against it, so that there were three equated lists of jokes arranged in equated order.

Main experiments. The Ss for the main experiments were 40 members of a graduate course in Social Psychology. They were asked to rate the three equated lists, which were arbitrarily called Lists A, B, and C, under different conditions.

TABLE II

SHOWING FOR EVERY LIST THE SCORE OF THE POOREST AND OF THE BEST JOKE, THE RANGE BETWEEN THESE EXTREMES, THE MEAN, STANDARD DEVIATION, AND COEFFICIENT OF VARIATION

List	Score on		Range	Mean	S.D.	Coef. var.
	poorest	best				
A	.5	2.1	1.6	1.57	.42	26.75
B	.6	3.0	2.4	1.64	.67	40.85
C	.6	3.2	2.6	1.92	.60	31.25

A 5-point scale was used. List A was presented in mimeographed form, and the Ss were asked to rate the jokes in absolute privacy, following directions similar to those given in the preliminary experiment. List B was read to the group as a whole, Professor Murphy and the writer alternating in reading the jokes while each member of the group scored them. List C was presented to the group visually by means of a lantern and slides.[4]

Then each joke was given a total weighted score as in the preliminary experiment, and each of these scores was divided by the frequency or number of Ss,

TABLE III

SHOWING THE DIFFERENCES BETWEEN THE VARIOUS LISTS IN MEAN SCORES AND STANDARD DEVIATIONS, AND THE RELIABILITY AND PROBABILITY OF THESE DIFFERENCES

List	Mean			Standard Deviation		
	Diff.	D/σ diff.	Prob.	Diff.	D/σ diff.	Prob.
B to A	.07	.56	71	.25	2.83	99
C to A	.35	3.02	100	.18	2.20	99
C to B	.28	1.97	98	−.07	.70	76

(40), in order to get the average score for each joke (see Table I). A crude estimate of the fullness of the laughter at each joke in the group situations was also made.

As Kambouropoulou noted,[5] jokes attended to with a view to grading are not considered funny. Table II shows that the lowest score on List A (the jokes rated in private) is 0.5; on List B (the ones presented vocally) 0.6; and on List C (the visual ones) also 0.6. The highest scores for jokes on Lists A, B, and C are 2.1, 3.0, and 3.2 respectively, giving a range of 1.6, 2.4, and 2.6. The average

[4] To check up on this experiment a similar group of Ss ought to be used and the lists reversed; perhaps List C being rated in private, List B presented visually, and List A read to the group. The lists of jokes used may be found in the writer's unpublished Master's Essay, filed in manuscript in the Columbia University Library.

[5] P. Kambouropoulou, Individual differences in the sense of humor and their relation to temperamental differences, *Arch. Psychol.*, 19, 1931, (no. 121), 1-83.

score of the jokes on List A is 1.57, on List B 1.64, and on List C 1.92, indicating that vocal jokes are considered slightly funnier than private jokes, and visual ones much funnier than either.

Table III shows that the reliability of the differences of the averages between B and A is 0.56, meaning that the chances are 71 out of 100 that the true difference is greater than zero. This figure for the difference between C and A is 3.02, showing the chances to be 100 in 100, and that for C and B is 1.97, giving 98 chances out of 100 that the difference between the true averages is greater than zero.[6] These figures were obtained by using the formula for uncorrelated material, as it was assumed that our three equated lists of jokes were three lists picked at random from an infinite number of equally correlated lists of jokes.

The S.D. for List A is 0.42; that for List B 0.67, and that for List C 0.60, with coefficients of variation of 26.75, 40.85, and 31.25 respectively. The reliability of the difference of the dispersions between B and A is 2.83, meaning that the chances

TABLE IV

SHOWING THE AVERAGE SCORE OF THE 8 POOREST AND 8 BEST JOKES ON LIST A AND THE SCORES OF THE EQUIVALENT JOKES ON LISTS B AND C

List	Average score of 8	
	poorest	best
A	.88	1.58
B	1.06	1.51
C	1.26	1.67
Av. B and C	1.16	1.59
Av. soc. improv.	.28	.01

are 99 in 100 that the difference between the true S.D. is greater than zero. Between C and A this figure is 2.20 representing 99 chances in 100, and between C and B it is 0.70, representing 76 chances in 100.

The average score of the 8 poorest jokes on List A is 0.88. The average score for the equivalent jokes is on List B 1.06; and on List C 1.26; combining B and C, it is 1.16. This shows an approximate improvement under social conditions of 0.28. The average of the 8 best jokes on List A is 1.58; for the equivalent jokes on Lists B and C it is 1.51 and 1.67 respectively. Combining B and C the score is 1.59, indicating that the social improvement of better jokes is, in this case, only 0.01 as against 0.28 for the poorer ones. The poor jokes, of course, have more room to improve than the good ones, but the scores of all the jokes in this experiment are so low that the tremendous difference between the facilitation of the poor and the good jokes cannot all be convincingly explained by this fact.

All the jokes that were laughed at considerably under the social conditions were rated from 2.2 to 3.2, which means that they were in the highest third of the lists. The scores do not show any appreciable sex differences, although most of the laughter seemed to have been initiated by the women *S*s.

Conclusions. These results suggest the following conclusions:

(1) Jokes presented to a group vocally are judged to be slightly but not reliably funnier than jokes judged in private.

[6] If the unreliability of the tests had been taken into account, the significance of the differences would have been even larger.

(2) Jokes presented to a group visually are judged to be funnier than jokes rated in private; and this difference is statistically reliable.

(3) Jokes presented visually seem to be funnier than jokes presented vocally, at least under the conditions of our experiment, which included telling the jokes (List B) in a rather uniform and undemonstrative manner.

(4) The ranges of scores obtained under the two social conditions are much wider than those of jokes rated in private; that is, the funniest and the least funny jokes are much farther apart under the social conditions.

(5) Social facilitation has much greater influence in raising the scores of the poor jokes than it has in raising the scores of the good ones. Perhaps the fact is that all of the poor jokes are facilitated while some of the good jokes are facilitated and others inhibited, thus not affecting the average; but this is just a suggestion and not a conclusion.

THE PSYCHOPHYSICAL DETERMINATION OF THE LIMITS OF PURE GRAY

By FORREST L. DIMMICK and GEORGE MCMICHAEL, Hobart College

The status of gray as an unique or fundamental color was first proposed by Dimmick,[1] and has been further justified experimentally by Dimmick and Holt[2] with a procedure that emphasizes the psychological or qualitative aspects but does not establish the region of pure gray. In the present experiment we have undertaken the determination of the limits as suggested by Rich.[3] We obtained the thresholds for black from gray and white from gray with 3 *Os* by the method of constant stimuli. Our gray was produced by the mixture of 'baryta' white and 'velvet' black papers. The black paper had a white valency of 3% as measured with the Kirschmann photometer. In order to maintain constant light conditions, the mixer was set up in a dark room and illuminated by an Ivanhoe daylight lamp (500 w. on 110 v. D.C.) hung one meter above and one meter in front of the mixer disk.[4] In front of the mixer was a medium gray screen which concealed the disk from *O* except when he was making a judgment. He was instructed as follows: "After a ready signal you will be shown a color disk. You are to report whether it shows any likeness to or hint of white or black. A judgment of 'neither' is permitted." After the signal, *E* raised the screen for 2 sec., exposing the stimulus to *O* who made his judgment of 'black' or 'white.' *E* immediately lowered the screen, shifted the mixer setting and gave a second exposure, and so on. Limens were obtained from 3 *Os* (undergraduate but with some practice in observation), who were given a long course of training in this type of judgment prior to the final series. This training consisted of making a number of similar liminal determinations for colors without, however, having pointed out to them the systematic implications of their judgments.

Results. Table I gives the limits in degrees of white of the region within which

TABLE I

WHITE-GRAY AND BLACK-GRAY LIMENS IN DEGREES OF WHITE

Baryta white paper standard. Numbers in parentheses include a correction of 3% for the white valency of the 'velvet' black paper as measured by the Kirschmann photometer.

	White	h	Black	h
G	95.12 (103.07)	.116	73.92 (82.5)	.1006
M	97.4 (105.28)	.0457	73.27 (81.87)	.0852
H	90.01 (98.11)	.0715	75.88 (84.4)	.078
Av.	94.18 (102.15) ±2.79		74.35 (82.91) ±1.35	

* Accepted for publication October 19, 1932.

[1] F. L. Dimmick, A note on the series of blacks, greys, and whites, this JOURNAL, 31, 1920, 301.

[2] Dimmick and C. H. Holt, Gray and the color pyramid, *ibid.*, 41, 1939, 284.

[3] G. J. Rich, Black and grey in visual theory, *ibid.*, 37, 1926, 123.

[4] Dimmick, An accurate differential color mixer, *ibid.*, 45, 1932, 798.

neither white nor black were reported. We may take the middle of this region, 80° (92°) white + 276° (268°) black, as the proper proportions for the best 'middle' or unique gray. This is slightly lighter than the gray used by Dimmick in his earlier work. Also, our region is narrower than that calculated from his results, viz., 20° as against 29°. This difference is probably due to the differences in the methods employed and in the practice of the *O*s in the particular type of judgment. For similar reasons it is not possible to make a close comparison of our results with the limens Rich gives, which were offered in substantiation of the earlier contention that quantitative determinations would show a region of gray that did not involve either black or white.

Summary. Our results agree with the contention that gray is an unique color quality, not a black-white composite, and establish 84° (±10°) white + 276° (±10°) black as the equation for neutral gray under our conditions of illumination.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY
OF VASSAR COLLEGE

LXII. Motor Factors in Voluntary Control of Cube Perspective Fluctuations and Retinal Rivalry Fluctuations

By M. F. Washburn and Annette Gillette

This study formed part of an experimental investigation comparing the alternations in perspective interpretations of an outline cube with the alternations in colored fields in retinal rivalry, both as regards their rate and as regards their controllability. It proved impossible to compare rates, owing to the factor of monocular dominance in rivalry. The present paper offers the results on relative controllability.

Method. The experimental conditions were as follows. The observers were 35 young women college students. The observations were made in a dark room: the *O* sat with her head in a rest; light came from a 50-watt ground glass bulb about 30 cm. above her head. For the experiments with the cube, a card carrying a drawing, in white lines on a dark ground, of a cube 1 in. a side (2.7 cm.) was placed against the wall on a table to which the headrest was attached; *O*'s eyes were about 60 cm. from this figure. During the periods of observation a metronome beat at 120 strokes per min., and *E* made a short pencil mark on paper with each beat. When *O* said, 'Down,' meaning that the cube was perceived as lying on the ground, *E* made the marks horizontal, and continued to do so until *O* said 'Up,' when *E* made the marks vertical until the next 'Down' signal. A period of observation lasted 2 min., after which *O* closed her eyes and watched the negative after-image of the cube. She reported appearances and disappearances of the after-image, and shifts of its up and down phases. *E* meanwhile continued to mark ½-sec. intervals in time with the metronome, in dots when no after-image was present, in horizontal marks while it appeared in the down phase, and in vertical marks while it appeared in the up phase, until the after-image finally disappeared. In the first of these observations *O* was instructed to make every effort (no method being suggested) to keep the cube in the down phase during the observation of the card. The after-image however was observed without effort to control it. In the next observation she was instructed to keep the cube as much as possible in the up phase; the after-image was uncontrolled as before. In the next observation the instruction was to observe the cube without effort to control it, but to try and see the after-image in the down phase. The next observation was also made with no attempt to control the phase of the cube, but with the effort to see the after-image in the up phase. Finally two observations were made, in one of which the instruction was to hold the down phase throughout both original and after image, while in the other the up phase was to be held throughout.

* Accepted for publication July 30, 1932.

Observations corresponding to these were made on rivalry. Upon a white card fitting an ordinary stereoscope a piece of standard red paper and a piece of standard blue paper of the same shape and size as the outline cube were pasted in positions so that they appeared to coincide when seen through the stereoscope. *O*, with her head in the rest, held the stereoscope to her eyes, which were closed while she waited *E*'s signal. *E* after giving *O* the signal to begin observing marked the half-seconds through the 2 min. interval, using horizontal marks when *O* said 'Red,' meaning that the figure looked red; vertical marks when *O* said 'Blue,' and dots when binocular color-mixture occurred and *O* said 'Purple.' When the 2 min. were over, at a signal from *E*, *O* closed her eyes and reported the appearances, disappearances, and colors of the after-images, *E* marking the ½-sec. intervals by vertical marks when the color was yellow and horizontal marks when it was green, and the half-seconds while no after-image was present, by dots. *O*'s instruction at the beginning of the first experiment on rivalry control was to hold the red phase in observing the card but make no effort to control the color of the after-image; in the second experiment to hold the blue phase but leave the after-image uncontrolled; in the third experiment to leave the observation of the card uncontrolled but try to see the after-image as green; in the fourth experiment to leave the observation of the card uncontrolled but try to see the after-image as yellow. In the next observation, as with the cube, the effort was to hold the blue phase in the original and the yellow phase in the after-image: in the last observation the red and green phases were to be held.

The method by which the success of these efforts at control was measured was as follows. As we have seen, there were two observations in which the effort was to hold the down phase of the cube while looking at the card. The average duration of the down phases under such down control was found. There were two observations in which the effort was to hold the up phase in the original. The average duration of the down phase under this up control was found, and the amount by which the duration of the down phases under down control exceeded the duration of the down phase under up control measured the efficiency of the control. This difference was expressed in terms of a percentage of the time of observation. The same method was used to measure efficiency of cube phase control in the cube after-image. In the rivalry experiments, the phases of binocular color-mixture giving purple had to be equally divided between the red and blue phases; otherwise the method of measuring control was the same as for the cube.

Results. The following results were obtained. *(1) Comparative controllability.* The average percentage of success in control was for the cube original 44.46; for the rivalry original 23.78, for the cube after-image 17.37, and for the rivalry after-image 13.07. The superiority of cube original control over rivalry original control was thoroughly reliable statistically, the difference being 5 times its standard error. Considering the results of individuals, we find that 83% of our *O*s controlled the cube original better than the rivalry original. The superiority of cube original control over cube after-image control also was statistically reliable, the difference being 3 times its standard error. The superiority of rivalry original control over rivalry after-image control was fairly reliable, the difference being such as to give between 97 and 98 out of 100 chances of a true difference greater than zero. There was no reliable difference between control of the cube after-image and of the rivalry after-image.

(2) Constancy of methods of control. It may be recalled that each *O* made two experiments where her task was to control the phases of the cube, and two where it was to control rivalry phases. The order of the *O*s in success of controlling the cube in the first observation corresponded with that in the second observation by a correlation of $+0.67 \pm .066$; their order of success in controlling rivalry phases in the first observation corresponded with that in the second observation by a correlation of $+0.66 \pm .07$. This indicates that most of the *O*s used approximately the same method of control in the second experiment as in the first.

(3) Individual differences. The standard error of the average cube original control for the group was 3.47; that of the average rivalry original control for the group was 2.37. The range of individual scores was for the cube original from 7.4% to 77%; for the rivalry original from —4.9% to 51.85%. This greater variability from one individual to another may be plausibly conjectured to be due to a greater number and variety of methods open for controlling the cube as compared with rivalry. The standard error of the average cube after-image control for the group was 8.5; that for rivalry after-image control was 4.9. The shorter duration of the after-image as compared with the two minutes' observation of the originals will account for the greater variability of after-image control measured in percentages of the total time; it was possible for after-image control to vary from plus 100% to minus 100% when the total duration of the image was short.

(4) Absence of a significant common factor. If these two cases of voluntary control had involved to any marked extent a common conative or will factor, there should have been a reliable positive correlation between an individual's success in cube control and her success in rivalry control. This correlation was, however, $+0.07 \pm .119$, indicating that the operating causes were different for the cube and for rivalry.

(5) Monocular dominance. There is one factor present in retinal rivalry that is absent from cube phase fluctuations. This is monocular dominance. For certain individuals the field of one eye habitually predominates over that of the other eye, and this might conceivably make rivalry phase control more difficult than cube phase control. To test such a supposition the *O*s were arranged in order of the degree by which their control of the cube surpassed their control of rivalry, and also in order of their amount of monocular dominance, this being measured by the size of the difference between the total time one color was seen during all 3 of the *O*'s uncontrolled rivalry observations, plus one-half the total time during which she saw purple; and the total time the other color was seen, plus one-half the purple time as before. The correlation found between these arrays was —0.06; hence it appears that the difficulty in controlling rivalry does not arise from resistance offered to such control by ocular dominance.

Explanation of results. In order to investigate the causes of the phenomena just described, we must consider the processes which naturally accompany them. *O*s were given no instruction as to how to control what they perceived: we wished to avoid all external suggestions. They were not asked at the end of a control period to report introspections as to their methods of control: we had no great confidence that the methods of which they were most conscious were those actually most effective in their control processes, and we wished to avoid autosuggestion. After the entire investigation was finished we sent questionaries to our *O*s asking simply, "When

you were asked to control the cube so as to prolong the up or down phases, what methods of control did you use? What methods did you use in the experiments with rivalry?" Twenty-five of our 35 *O*s answered these questions. Of course little dependence can be placed on their replies.

Our experimental results indicate that our *O*s used for control of the cube original phases some method or methods different from and much more effective than the methods they used for controlling rivalry original phases. Now there is one process accompanying observation of cube perspective that has obviously much less significance in the observation of rivalry changes. This is *actual movement of the eyes, shift of fixation.* It has been held that shifting fixation is the main factor in determining the change of perspective interpretation. The point one looks at appears nearer than when it is not directly looked at. If one moves one's eyes along an edge of the cube, that edge seems to come nearer. Our *O*s were at the outset of the experiments told to fixate a certain corner of the cube, but the instruction was not repeated with emphasis and probably could not have been followed in any case. It is only in control of the cube original that fixation or eye-movements can favor a particular phase. Obviously they cannot do so in rivalry or in any after-image, for the after-images move with the eyes. We have certainly here a factor capable of explaining the marked superiority of cube original control over other controls.

Our results indicate further that more methods are open for control of the cube original than for control of rivalry or after-images. One factor in cube control is apparent to ordinary introspection. *Movement ideas* are present during observation of cube fluctuations: the ideas of sitting down on the cube, of reaching down to handle it, of passing the hand over the surface in the "down" phase; or of standing under it and looking up, of reaching up to touch its under surface in the "up" phase. These movement ideas, as has now been experimentally demonstrated by Jacobson,[1] are accompanied by slight actual contractions of the muscles concerned. No movement ideas have any such tendency to occur in connection with rivalry.

The cube then has two methods which favor the prolongation of a specific phase and which are not available for rivalry control. Another motor process which occurs in the effort to control phases is *making eye movements,* especially *blinking,* when the undesired phase is present; this always tends to change the phase; conversely, *preventing eye-movements* helps to hold a phase. This was the method mentioned oftenest in the questionary answers. It is available for rivalry as well as for the cube.

Subvocal verbal reactions, saying to oneself 'up,' 'up,' or 'blue,' 'blue,' while trying to restore or maintain a given phase, undoubtedly occur and might be effective through the association of the movements they involve with the muscular contractions peculiar to the phase desired. Only 3 of the 25 *O*s who answered the questionary on methods of control mentioned such verbal reactions in connection with cube control; 2 of these reported them also in connection with rivalry control. But it seems probable that all the control in our experiments depended for its origin on verbal reactions, since it was derived from the *O*'s acceptance of verbal instructions.

[1] E. Jacobson, Electrophysiology and mental activities, this JOURNAL, 44, 1932, 677-694.

General muscular tension, which is probably what our *O*s meant when they mentioned "concentration" as an aid, may quite conceivably help to maintain a desired phase against change by checking disturbing movements.

There are then 5 muscular processes that assist control of the cube: eye movements, the slight muscular contractions underlying movement ideas, avoidance of blinking when a desired phase is to be held and blinking to get rid of an undesired phase, verbal reactions, and general muscular tension. The first two of these, eye movements and movement ideas, are absent in rivalry control. Eye movements cannot be used for control of the cube after-image; blinking cannot be effectively used to change the phase of an after-image because it tends to make the after-image itself disappear. Thus it is clear that cube control has as its service more methods than has rivalry control; which would explain its greater variability in different individuals. Cube after-image control can use movement ideas, since one can think of reaching down, sitting down, and so forth when one sees the cube after-image in the down phase, but the tendency of the after-image to disappear at any full eye movement diminishes the efficacy of control. The same factor of instability of the after-image must explain the inferiority of control of the rivalry after-image to control of the rivalry original. And the fact that cube after-image control, though it has the factor of movement ideas at its service, is so little better than rivalry after-image control, can be explained by the inferiority in clearness and duration of the cube after-image as compared with the rivalry after-image. The white lines of the cube offered a weaker stimulus than the colored areas on the rivalry card.

These experiments throw no direct light on the cause of perspective and rivalry fluctuations. Since the control processes appear to be motor, like all other control processes, it would appear that the inevitable relaxing of control must be due to muscular fatigue, but why this relaxing should be periodic is another problem.

A motor psychologist would describe the whole process involved in control of these phenomena of visual perception as follows. There is present in the observer at the outset the drive of submission to the directions of the experimenter. Physiology cannot yet explain to us in terms of glandular secretions and hormones this social drive; it is certainly the opposite of the fighting attitude and may be supposed to involve low adrenal activity. At any rate it produces a condition of relaxed muscular tension; the *O* has no tendency to set up attitudes antagonistic to the movements, full or tentative, suggested by the words of the *E*.

The nature of these movements and the differences among those available for cube original control, rivalry original control, cube after-image control, and rivalry after-image control have been sufficiently described. By certain philosophers and psychologists the existence of mind as a dynamic entity capable of directly controlling its own processes has been postulated. The absence of any correlation between cube perspective control and rivalry phase control, and the marked superiority of that form of control which is accompanied by the most various motor processes, suggest strongly that in these phenomena no other causal agency operates except the familiar physical one of the interaction and interference of movements.

LXIII. Stereoscopic Binocular Fusion in the Original Impression and in the Negative After-Image

By M. F. Washburn and Dorothy Lucile Smith

During some experiments reported in Number LXII of these Studies, where observations were made upon retinal rivalry of small colored fields and upon the ensuing negative after-images, it was noticed that while binocular mixture of the original red and blue fields, resulting in a purple field, often occurred, there was not a single report of binocular mixture of the green and yellow negative after-images. It seems remarkable that while with the eyes open the processes on corresponding points should fuse part of the time, this fusion should be almost if not quite absent on the part of the after-image processes on the same points. The difficulty of observing it in the faint and unstable after-image must of course be reckoned with. The problem suggested itself of finding whether the binocular fusion of dissimilar images which occurs in stereoscopic vision is also diminished in the negative after-image. The present paper describes an investigation of this problem.

In a dark room the *O* sat with her head in a rest, light coming from a 50-watt ground-glass bulb about 30 cm. above. The stereoscopic slide used was one which when looked at through the stereoscope appeared as a truncated cone with its smaller end directed towards the *O*. The right-hand design showed a larger circle with a small circle within it towards the left; the left-hand design showed the same larger circle with a similar small circle within it towards the right. The design was in black lines on a white ground; we experimented also with this design in white on a black ground, and with one showing a truncated pyramid in white on black and in black on white; but the black lines of the truncated cone gave us the clearest after-images.

In the experments where the stereoscopic slide itself was observed, *O* was directed, after placing the slide in the stereoscope, to adjust its position until it "looked like a cone projecting out toward" her; then while holding the stereoscope directed slightly downward to get the best light from above, and keeping her head fixed in the rest, she was instructed to fixate the small circle which represented the truncated end of the cone, and to watch for any shifts in its position. When they occurred she was to say, "Right" when the small circle looked displaced towards the right (dominance of the left-eye image), "Left" when the small circle looked displaced towards the left (dominance of the right-eye image), and "Center" when the small circle appeared to be in the center of the large one (binocular stereoscopic vision). *E* recorded the time of these changes by making short marks on paper in time with a metronome ticking at 120 per min.; when *O* said, "Left," *E* made the marks horizontal; when *O* said, "Right," *E* made vertical marks; when she said, "Center," horizontal marks one under another were made.

In the experiments where the negative after-image was observed, *O* looked steadily at the small circle for approximately 2 min., and then at a signal from *E* closed and covered her eyes and watched the after-image, reporting its appearances and disappearances and whether the small circle appeared displaced towards the right or left, or in the center. *E* recorded these changes as in the experiments where the original card was observed, using dots to mark the half-seconds when no after-

image was present. In these experiments the period during which *O* looked through the stereoscope at the card was varied from one *O* to another, in order to find what exposure time gave the *O* clearest after-images: these times varied from one to five minutes.

In both the original and after-image experiments four observations were usually made by an *O* during an experimental period. There were 19 *O*s, all young women college students.

Results. The total time in seconds during which the small circle appeared in the center of the large one, showing that binocular vision was present, was found for observation of the original and of the after-image in the case of each *O*, and was expressed as a percent. of the total time during which the *O* observed the original in the one case and the after-image in the other. These percentages were as follows:

Observer	*Original*	*After-Image*
H	95.0%	.9%
T	90.5%	14.6%
A	66.0%	10.8%
R	85.2%	40.4%
N	100.0%	54.0%
M	88.0%	16.2%
B	68.0%	0.0%
G	87.0%	29.0%
F	47.8%	49.0%
L	99.5%	46.4%
S	77.0%	95.0%
Z	41.8%	17.6%
Ch	15.0%	38.0%
Ca	100.0%	20.9%
Se	88.6%	0.0%
Le	58.0%	0.0%
Ga	85.0%	56.5%
Oa	92.8%	35.0%
Oe	63.8%	90.0%

Thus for 15 of our 19 *O*s or 79%, binocular fusion occurred for a longer time in the original than in the negative after-image, by differences ranging from 94.1 to 24.2%, with the median at about 58%. The other 4 *O*s showed better fusion in the after-image than in the original, by differences ranging from 26.2 to 1.8%.

These figures so far as they go indicate that *the after-image of a stereoscopic impression shows decidedly less tendency to binocular fusion of the two dissimilar retinal images than does the original impression.* Yet the original images and the after-images occur in the same retinal regions. If binocular fusion is secured merely by having the images of an object fall on corresponding points in the two retinas, then it should occur as readily in the negative after-images as in the original images. It is impossible to avoid the conclusion that the situation in binocular vision is not so simple as has been supposed. May the fusion of unlike images into one in stereoscopy depend partly on *conditions which suggest unity of motor response,* for instance of the response of reaching towards and grasping the single solid object from which the images are caused? This response is lacking under the general conditions of after-image observation: the image appears to float in the air, either in the dark field of the closed eyes, unassociated in our experience with any successful grasping responses, or as a faint vision in the field of the open eyes, unreal by comparison with the surrounding objects.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

VI. Color Preferences of 1279 University Students

By Wm. E. Walton, Ruth B. Guilford, and J. P. Guilford

In common with many other laboratories of psychology, the University of Nebraska laboratory gives in its elementary experimental course a study of color preferences using the method of paired comparisons. Since procedures have been devised for computing scale values in the method of paired comparisons, the experiment has taken on added significance. It is possible to show the student how his likes and dislikes can be measured scientifically. As in many elementary experiments where quantitative results are obtained, however, there are very few available norms with which to compare the results of a new subject or group of subjects. It is the main purpose of this study to supply such norms based upon a large number of subjects used under a variety of conditions. Since the Milton Bradley colored papers, which were used throughout these experiments, are rather generally used for studying color preferences, the norms should be of value to other laboratories, provided conditions other than the color stimuli used, are comparable. During the course of preparing the norms, certain sex differences, yearly variations, and other constant tendencies appeared, and so the study was broadened in scope in order to take into consideration these secondary problems.

The stimulus-material that was used throughout the 14 years during which our results were obtained, consisted of the best chromas of the 18 spectral hues in the Milton Bradley series. These colored papers were cut into 9 cm. squares and glued side by side in pairs, 2 cm. between squares, on gray cardboards 20 x 30 cm. The gray background had a dull finish with a brightness value of 5 on the Munsell color scheme. There were 153 such pairs of stimuli since each one was paired with every other one. The order of presentation of the pairs was the same as that worked out by Titchener.[1] Every color appeared equally often on the right and on the left. The stimulus-cards were set up in daylight illumination in front of a large gray cloth background. The *Ss* were given the experiment in groups ranging from 5 to about 15 in number. All *Ss* were seated as near to the stimulus-cards as space would permit. Every card was exposed for 5 sec. and *S* was instructed to record on a prepared blank which of the two colors (designated by a key) was more pleasing to him.

The results from these experiments had been carefully saved from the school year 1910-11 through the year 1920-21, and again from 1928-29 through 1930-31. From 1910-11 through the year 1918-19 they were given under the supervision of

* Accepted for publication December 10, 1931.

[1] E. B. Titchener, *Experimental Psychology*, I, Pt. 1, 93.

Dr. H. K. Wolfe, through 1920-21 under the direction of Dr. Winifred Hyde Dodd, and during the last 3 years they were supervised by the senior author. While the actual experiments were conducted by laboratory assistants, and we are not prepared to insist that the conditions described above were always scrupuously carried out, we do know that for the last 6 years for which we have results, the assistants were carefully trained and instructed in the procedure of this experiment. There is no reason to believe that the earlier experiments were any less carefully supervised. All record sheets were discarded if the *Ss* omitted any judgments or showed in any other way that they did not coöperate in the experiment. A number of such records had to be discarded every year. We have tested the reliabilities of the

TABLE I

SHOWING THE AFFECTIVE VALUES OF THE DIFFERENT COLORS FOR THE MEN AND WOMEN SEPARATELY AND FOR THE GROUP COMBINED

Color	Affective values		
	464 men	815 women	1279 combined
VR	.128	.172	.150
R	.495	.602	.549
OR	−.040	−.457	−.249
RO	−.360	−.431	−.396
O	−.064	−.506	−.285
YO	−.545	−.460	−.503
OY	−.563	−.238	−.401
Y	−.207	−.192	−.200
GY	−.609	−.478	−.544
YG	−.307	−.221	−.264
G	.418	.151	.285
BG	−.089	.207	.059
GB	.194	.567	.381
B	.763	.304	.534
VB	.497	.405	.451
BV	−.142	.200	.029
V	.308	.437	.373
RV	−.190	.050	−.070

measurements for each year, and with few exceptions find them very acceptable. The coefficients of reliability are shown in Table II.

The method of computing scale values for the colors has been described elsewhere by the senior author.[2] The probable error of the normal cumulative frequency curve has been used as the unit of measurement instead of the standard error. Since the number of stimuli remained the same throughout the experiments, the scale values are comparable from one year to another.[3] The data from the men and women were treated separately and were also combined. The 756 scale values which were thus computed are not published, since the space required would not be justified by the importance of the single values. The affective values for a few of the colors showed distinct trends or changes during the course of years, and these trends are

[2] J. P. Guilford, The method of paired comparisons as a psychometric method, *Psychol. Rev.*, 35, 1928, 499 f.

[3] Guilford, Some empirical tests of the method of paired comparisons, *J. Gen. Psychol.*, 5, 1931, 64-77.

given in the form of curves (see Figs. 1 and 2). The norms that we undertook to find are listed in Table I.

The scale values for either sex during any one year usually ranged from about +1 P.E. to —1 P.E. When the various years are combined, these ranges decrease considerably, due principally to the fluctuation of the values from year to year. In Table I, the total range for men is 1.372, and for women it is 1.108. Elsewhere, the senior author has used the range, or better, the standard deviation of the whole group of scale values, as a measure of the homogeneity of the group of *Ss* from which the judgments were obtained.[4] These results entirely justify that procedure. When the results of the two sexes are combined, as in the third column of Table I, a further reduction in range occurs, but not very much beyond the already narrow range for the women alone. The range for column three is 1.093 as compared

TABLE II

RELIABILITY COEFFICIENTS OF THE SCALE VALUES FOR THE DIFFERENT YEARS AND FOR THE TWO SEXES

Year	(N varies)				(N equals 100)	
	Men	(N)	Women	(N)	Men	Women
1910–11	.988	27	.578	18	.998	.862
1911–12	.876	30	.975	90	.980	.976
1912–13	.902	49	.978	85	.950	.982
1913–14	.850	36	.979	78	.951	.984
1914–15	.726	46	.900	90	.850	.909
1915–16	.929	60	.930	69	.946	.950
1916–17	.111	44	.845	115	.221	.825
1917–18	.360	13	.965	50	.834	.977
1918–19	.683	14	.954	50	.939	.976
1919–20	.958	68	.900	78	.970	.918
1920–21	.761	28	.618	27	.917	.857
1928–29	.380	6	.608	10	.910	.939
1929–30	.952	26	.888	32	.971	.959
1930–31	.854	17	.185	23	.943	.489
Means	.738	33.1	.807	58.2	.884	.900

with 1.108 for the women alone. Even when sex differences are eliminated, the differences in scale values do not vanish, which indicates that there is a common basis of feeling for different colors in this group of 1279 students. This group is surely large enough to be called a random sampling of the students in one part of the country. Some of the results, to be discussed in more detail later, indicate that the so-called culture patterns, fashions, fads, or other social factors may be the cause of wide differences in color preferences from one year to another and between the two sexes. Even when these factors are cancelled out, more or less, by the pooling of the results from nearly 1300 *Ss*, there still remains sufficient agreement upon color preferences to indicate a basic, biological cause of likes and dislikes for colors.[5]

[4] Guilford, Racial preferences of a thousand American university students, *J. Soc. Psychol.*, 2, 1931, 179-204.

[5] Incidentally, it seems to be quite clear from Table I that the more familiar colors, like red, orange, yellow, green, blue, and violet, have slightly higher affective values than their less familiar neighbors in the spectral series. This is much more true of the men's values than of the women's.

We turn next to the question of the reliability of the measurements. This is important, in view of the somewhat uncertain conditions under which some of the results were obtained. It is of interest, also, to compare the self-consistency of groups of men and women *Ss*, and the consistency of the results from one year with those from other years. To this end we have found the reliability coefficients for each sex for each year. This was done by splitting the data into two halves, the *Ss* being divided at random, computing two sets of scale values, correlating the two sets of values, and applying the Spearman-Brown formula. This application of the Spearman-Brown formula has been tested empirically for similar types of data and it has been found to work with perfect satisfaction.[6] Since the reliability of any such set of measurements increases as a rule with the number of judges, and since the number of *Ss*, both men and women, varied considerably from year to year, the reliability coefficients alone do not give us a fair comparison between the different years and the two sexes. We have taken the liberty to extend the use of the Spearman-Brown formula still further, and to predict for each set of values the reliability coefficient which would have obtained if there had been exactly 100 *Ss* in each group of judges. The results, both the reliability coefficients for varying numbers of *Ss* and those estimated on the basis of 100 *Ss*, year by year, are presented in Table II.

The measurements can be considered very reliable since the coefficients are above 0.90 in 5 of the years for the men, and in 8 of the years for the women. They are totally unreliable in at least 3 years for the men, and in 1 year for the women. This unreliability is largely attributable to the small numbers of *Ss* for most of those years, for when the Spearman-Brown formula is applied to estimate the reliability for 100 similar judges, the coefficients are nearly all in the 90's. Only one year has wholly unreliable measurements for the men and for the women. For the former, this is the year 1916-17; for the latter it is 1930-31. Since the original coefficients upon which these estimates are made are low and therefore have large probable errors, this may be due to chance. Therefore no interpretations will be attempted. When one compares the means of the coefficients of reliability, taken without regard to the number of *Ss*, it would seem that the women tend to give more reliable measurements, that is to say, they agree with one another better. But when the number of *Ss* is held constant, as in the last two columns of Table II, the averages are approximately equal. One might expect that women would show less agreement among themselves and therefore have less reliable measurements, as is indicated by their narrower range of scale values. The narrower range may after all be a better indicator of individual differences in preferences, whereas the reliability coefficient may indicate the adequacy of the sampling of the whole group. This would mean that although there are more personal, individual tastes among the women, it is just as easy to get a representative group of them as it is of the men by taking a random sampling of the same number. The indications in Table II lead to the conclusion that in order to secure reliable affective values for any relatively homogeneous group of *Ss*, one should use not less than 50 *Ss* chosen at random.

Will men, or women, disagree more among themselves from one year to another than they do within the same year? In order to obtain a rough answer

[6] The empirical proof for this has not yet been published.

to this question, we found the means of the inter-yearly correlations of scale values. By means of Kelly's formula[7] it is possible to find the mean intercorrelation between sets of observations without finding all the separate coefficients. We have made use of this procedure, first finding *rho* and then translating this into the Pearson r. For the men, the mean of the inter-yearly correlations was 0.802, and for the women it was 0.681. If these are compared with the means of the reliability coefficients in Table II, an interesting conclusion is suggested. Those two means are 0.738 and 0.807, respectively. The scale values for the men correlate just as well from one year to another, on the average, as they correlate with themselves in the same year. For the women, however, there is a drop of about 0.13 between the mean reliability and the mean inter-yearly correlation. If these estimates are

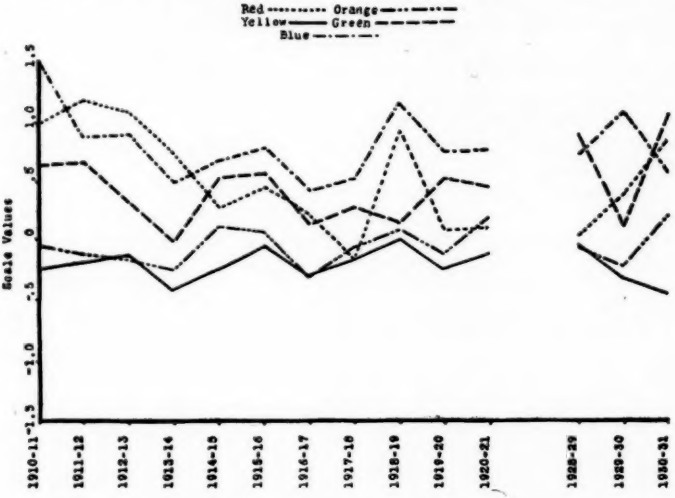

FIG. 1. SCALE VALUES OF FIVE COLORS FOR 464 MEN FROM 1910 TO 1931

correct, it means that women are more inclined to shift in color preferences from year to year, and that they are inclined to 'follow the herd' in doing so. This fact was also verified by Walton and Morrison in a study with colored lights.[8]

What can be said as to the reliability of the norms that have been set up in Table I? Applying the Spearman-Brown formula again, we have predicted the reliability coefficients for all the men (464) and for all the women (815). The coefficients are 0.972 and 0.987 respectively. The second value might need to be slightly reduced, due to the yearly fluctuations in color preference of the women. The index of reliability, or the correlation between the actual measurements and

[7] T. L. Kelley, *Statistical Methods*, 1924, 218.
[8] W. E. Walton and B. Morrison, A preliminary study of the affective values of colored lights, *J. Appl. Psychol.*, 15, 1931, 298.

the true thing that we are trying to measure, in this case affective values for colors, under the conditions which have prevailed during these fourteen years of experimentation, is equivalent to the square root of the coefficient of reliability. In these two cases the indices of reliability would be 0.986 and 0.993 respectively.

Earlier in this report we mentioned distinct trends or alterations in affective values for single colors. The most decided of these, perhaps, was in the case of red. Beginning near the top of the list for agreeableness for both men and women in 1910-11, it declined in relative value almost continuously, until a minimum value was reached in the year 1917-18. A sharp rebound from this minimum occurred in the year immediately following and lasting for the one year only. When next we find the value of red in 1928-29 and the two years following, it is climb-

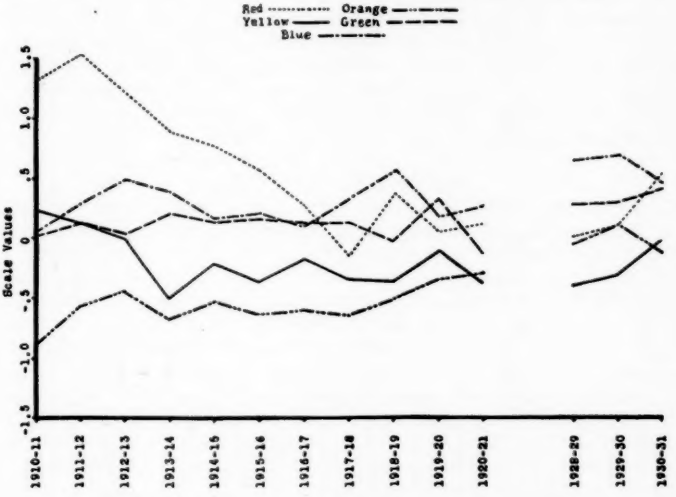

FIG. 2. SCALE VALUES OF FIVE COLORS FOR 815 WOMEN FROM 1910 TO 1931

ing back to a higher value. The agreement is so close between the two sexes in this respect, that one is tempted to look for some underlying reason. The regular decline and recovery of red could hardly be due to chance. Such irregularities in these results indicate the importance of continuing a study of color preferences and the like over a period of years and under varying conditions. Only in that way can the factors controlling color preferences be isolated and discovered.

Figs. 1 and 2 show the yearly fluctuations in several of the principal colors: red, blue, green, orange, and yellow. One of the persistent sex differences is the greater value of orange as compared with yellow for the men and the reverse relative evaluation of those two colors by the women. The two sexes, on the other hand, agree persistently in the relative values of blue and green. Although not all the 18 colors are represented in the curves, some idea can be gained con-

cerning the shift in total range of values from year to year. When all 18 colors are considered, the following rather striking shifts in total range appear. For the men, the maximum range occurs in the first two years. The range then progressively narrows down to a minimum in the year 1916-17. From that year it again widens to a sub-maximal degree and remains fairly constant until the end. For the women, also, there is a maximal range at the start, and a progressive narrowing that does not reach a minimal point until the year 1920-21. The ranges for the last three years are relatively wide again. These shifts in range are seemingly unrelated to numbers of *Ss* in the groups, and only slightly related to variations in coefficients of reliability. If the assumption is correct, that the total range measures the homogeneity of opinion in the group, one has some indication of trends in 'individualism' as expressed in color preferences in Figs. 1 and 2. Fluctuations from year to year such as we have just been discussing could hardly be due to gross alterations in experimental conditions. Chance alterations in experimental conditions could hardly produce continuous trends that persist over a period of years. We would again insist upon the importance of follow-up studies with accurately kept records from year to year. Only in this way can some of psychology's most persistent problems be solved.

Summary and Conclusions

Results from experiments upon color preferences had been accumulated over a period of 14 years. The 18 Milton Bradley colored papers were used as stimuli with a total of 464 men and 825 women *Ss*. The following conclusions are indicated in the results.

(1) Very reliable norms, as tested by self-correlations, were established for the affective values of these 18 colors under varying conditions.

(2) Certain differences between the two sexes in color preference persist rather consistently from one year to another. For example, men usually prefer orange to yellow, but for women the preference is reversed.

(3) Certain colors showed a continuous shift in affective value during the course of years for both sexes. For example, red decreased continuously in value from 1910 to 1918, its value was uncertain for a time, and it appears now to be rising to its former high position on the scale.

(4) Women's color preferences are somewhat more inclined to fluctuate from one year to another than are the men's, but individual differences in color preferences within any one year are about equal for the two sexes.

(5) Persistent differences in affective values for colors, in spite of fluctuations from year to year and in spite of sex differences indicate that, in addition to social conditioning and other environmental factors, there are undoubtedly deep underlying biological factors that help to determine color preferences.

APPARATUS

A DEVICE FOR VARYING AND CONTROLLING THE ENTRANCE PUPIL

By C. E. Ferree and Gertrude Rand, Johns Hopkins University

The description of the device to be presented will be prefaced by a brief discussion of the factors which influence the size of the natural pupil and of the effects of size of pupil on the formation of the image and on the visual response.

Among the factors which are known to affect the size of the pupil the following may be mentioned: age, errors of refraction, distance of object, intensity of light, area and location of luminous surface in the field of view, drugs, other sensory stimuli, mental and emotional states, and pathologic disturbances. All of these factors are interacting in their influence. The effect of any one is modified by the presence of the others. Any statements that are made with reference to factors have, therefore, to be made in a very general way.

(1) Age. At birth the pupils are very small. They become larger during childhood and still larger during the period of adolescence. From adolescence to middle age they are of medium size and in old age they again become smaller. The causes which are assigned for these changes are largely a matter of conjecture. For example, the dilator muscles of the iris are under the influence of the sympathetic nervous system. In early childhood these muscles are inactive because of the generally undeveloped state of the nervous system. During the period of adolescence, however, their activity is greatly increased, due probably to overstimulation by an unstable and highly sensitive sympathetic system. In middle age a better balance is established between the action of the sphincter and dilator muscles and the pupil becomes of medium size. But in old age the dilator muscles again become inactive and the pupil returns to the smaller sizes found in early childhood. In addition to changes of innervation there are also present in old age, according to various writers, a loss of pliability of the iris due to vascular sclerosis, a thickening of connective tissue behind the sphincter muscle and the presence of hyalin connective tissue in the border of the pupil. All of these would tend to prevent dilatation. However, whatever the cause, as a partial compensation for the loss of accommodation, the hyperopia and other defects in focussing which come with advancing age, the small pupils of old age serve a very important purpose in the economy of vision.

(2) Errors of refraction. Myopes, particularly young myopes, have in general larger and hyperopes smaller pupils than emmetropes. This may be due in part to an associative relation with the function of accommodation, the myopes requiring less and the hyperopes more accommodation than emmetropes.

(3) The adjustment of the eye for distance. As the object approaches the eye,

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the pupil contracts. This is a valuable feature in the accommodative process for change of distance, and adds greatly to the clearness and speed of seeing when the eye is compelled to change its adjustment from far to near. In making this adjustment the lens may perhaps change too much or too little to give the best focus, but the change in the pupil is always in the direction of clearer imagery. To clean up after the lens in the process of accommodating for distance is a very important function of the pupil. The pupil as an aid to clear seeing is especially needed as the near point of vision is approached. The distance of the near point is strongly affected by size of pupil. This is true more particularly in middle age and in early presbyopia.

(4) Intensity of light. The reaction to intensity of light is strongly influenced by the presence of other factors. The most important of these from the standpoint of experimental control are perhaps size and distance of illuminated area or surface and its location in the field of vision. With a field of view bounded by surfaces of nearly uniform coefficient of reflection of the order of 75-78 percent we found in a study of 6 observers ranging between 20 and 30 years of age, that the knee of the curve for the reaction to intensity fell between 10 and 20 foot-candles. Below these values the curve was steep; above them the change in the slope of the curve was comparatively slight.[1]

(5) Area and location of luminous surface in the field of view. Area of field as well as the intensity of its illumination exerts an important effect on the size of the pupil.[2] The effect, however, is not so great for change of size as for change of intensity. Also for the range of size and intensities which we used there is no pronounced knee of the curve for size. The location of the illuminated surface in the field is a further important factor. Hess, for example, has said that the pupillomotor fibers are all confined to the macular region of the retina and that the stimulation of peripherally located fibers has no effect on the size of the pupil.[3] Our results on the influence of size of illuminated field show, however, that there is a marked effect of increase in size far beyond the limits of projection of the macular area into the field and that the effect is greater through that part of the retina which has increased sensitivity to light, at least in dark adaptation, than it is through the more centrally located portions of the retina.

(6) Drugs. With reference to effect on size of pupil, drugs may be divided into two classes: miotics, which produce a contraction of the pupil and mydriatics which cause the pupil to expand. Among the most commonly used drugs of the former class are eserin, pilocarpin and muscarin; among the latter, atropin, homatropin, belladonna, cocaine, adrenalin, scopolamin, hyoscomin, duboisin, etc. Unfortunately for experimental purposes, also unfortunately in some cases for use

[1] C. E. Ferree and G. Rand. Relation of size of pupil to intensity of light and speed of vision, and other studies, *J. Exper. Psychol.*, 15, 1932, 37-55.

[2] Ferree, Rand, and E. A. Harris, Intensity of light and area of illuminated field as interacting factors in size of field, *ibid.*, (in press).

[3] C. Hess, Untersuchungen über die Ausdehnung des pupillomotorisch wirksamen Bezirkes des Netzhaut und die pupillomotorischen Aufnahmeorgane, *Arch. f. Augenhk.*, 58, 1907, 182-205; Investigations concerning the extent of the active pupillomotor area of the retina and concerning the pupillo-motor receiving organs, *Arch. Ophth.*, 38, 1909, 131-146; Das Differential-Pupilloskop: Eine Methode zur messenden Bestimmung von Störungen des Pupillenspieles. *Arch. f. Augenhk.*, 80, 1916, 213-228.

in treatment, these drugs do not affect the pupil alone. Pilocarpin, for example, in non-presbyopic eyes produces for a considerable time a well pronounced myopia and atropin and homatropin produce cycloplegia as well as mydriasis. The action of some of the mydriatics on accommodation, however, is slight.

(7) Other sensory stimuli and emotional states. Size of pupil is affected by sensory stimuli other than light. The tendency of these stimuli is in general to dilate the pupil. As an effect of the various mental and emotional states both contraction and dilatation may occur. In sleep in spite of the closed lids and dark adaptation the pupil is contracted. In light and medium general anaesthesia the pupil is contracted and immobile. In dangerous anaesthesia it is dilated and fixed. It is also dilated in epilepsy and convulsions and in coma and in death.

(8) Pathologic disturbances. Among the classes of effects in the reaction of the pupil that are produced by pathologic conditions may be mentioned spastic and paralytic miosis and mydriasis, anisocoria (unequal pupils), hippus (rapid fluctuation in breadth of pupil), the Argyll-Robertson pupil (absence of light reflex with retention of reaction to distance), and cyclic contractions and dilatations. While the reactions of the pupil in general constitute a most important and voluminous chapter in the diagnosis of nervous diseases, there seems to be no disease which can be diagnosed from a pupil sign alone. There is, however, some controversy on this point.

The influence of size of pupil on the visual response may be briefly summarized as follows: (1) There is an effect on the intensity of the image formed. The amount of light condensed into the image should be in direct proportion to the square of the breadth of the pupil. This applies not only to the light from the object or stimulus but also from the surrounding field, and thus affects the state of adaptation and sensitivity of the entire retina and both directly and indirectly the response of any given part of the retina.

(2) There is a very great effect also on the clearness of the image. This effect is produced in the following ways: (a) The breadth of the cross section of the cone of light from each point in the object is changed. As it is decreased, for example, the blurring of the image caused by the presence of diffusion circles is decreased. Always an important factor in the sharpness of the image formed, this effect becomes especially pronounced when there is a fault in the focussing action of the refracting system. As will be shown in a forthcoming paper, pupils of the order of 1-1.25 mm. in breadth render the eye practically independent of errors of refraction, also practically independent of changes in accommodation. These are very important incentives for the use of an artificial pupil in many types of experimental work. The facts also have many other practical applications.

(b) The proportion of light diffracted near the edges of the pupil to the total light and the breadth of the diffraction patterns in the image, central disk, and concentric rings, are also changed. This effect tends to counteract the beneficial action of the contraction of the pupil noted above. It does not, however, become of considerable consequence until unusually small sizes of pupil are reached. In recent determinations we have found, for example, that the values which have been assigned to the optimum breadth of pupil are much too high. The error in former determinations seems to have been due in large measure to the use of an artificial pupil too far in front of the eye.

(c) The aberration effects and other irregularities of focussing resulting from the use of the periphery of lens and cornea are excluded by pupils of small and medium size. The importance of these effects, while always great, varies considerably even in normal eyes. The spherical aberration ordinarily found in a refractive system as strong as is present in the eye is enormous. While the flattening of the lens and the decrease in the refractive index towards the periphery act as a corrective, there still remains a considerable amount of this defect in all eyes. Also with advancing age changes in density occur which are unequally distributed throughout the lens. Thus by decreasing the size of the diffusion circles in the image and by limiting the aperture to the most favorable portions of the refractive system the pupil acts as a final correction on all defects in focussing.

(3) Reduction in the size of the pupil may further exert a secondary effect on the clearness of the image. That is, in passing to the retina a certain percentage of light, variable from eye to eye, is scattered by internal reflections within the media. This scattered or unfocussed light tends to blur or confuse the image and when present in sufficient amount causes the phenomenon known as veiling glare. By reducing the area of the surface of the cornea acted upon, the cross section of the beam throughout and the variety of angles at which the rays of light may strike the bounding surfaces of the media and the various reflecting particles and surfaces which are to be found in the media, a small pupil over and above the effect on the amount or intensity of light reaching the retina may serve to reduce the overlay of scattered light in the image. In any event the use of such an aperture before an eye that would be classed as emmetropic noticeably clears up vision under light of moderate and high intensities in spite of the reduction in the intensity of the useful light in the image. This may not be due entirely to an improvement in the focussing.

An interesting objective demonstration of the effect of size of pupil on clearness of image for lecture and class room use can be made with a projectoscope, a lantern slide of a test letter such as the broken circle, and a plate or stop with an aperture of suitable size. An iris diaphragm is convenient for this purpose inasmuch as with it the effect of different amounts of reduction in size can be shown in series. Two cases may be made of the demonstration: when the focus is correct, or approximately so and when the image is so badly blurred as to be entirely formless and unrecognizable. In the former case some improvement in the clearness of the image may be noticed and in the latter the image can be cleared up with even a moderate reduction in size of the lenticular aperture. A very good image can be obtained with a small aperture even when the focus is so poor that the effect is merely a fairly uniform patch of projected light.

When an artificial pupil is employed, it and not the natural pupil should, of course, be treated as the entrance pupil. Unfortunately it has to be located in front of the refracting system and is usually placed much too far in front to serve well the purpose for which it is intended.

Two important uses for artificial pupils may be noted: (1) in a general examination and rating of eyes; and (2) in experimental work for various purposes of variation and control.

(1) The examination of the eye is usually begun with a test for acuity. Low acuity may be due to several causes such as refractive defects, low coefficient of

transmission of the media of the eye and defects in the sensorium. It is of very great advantage to be able to know at the beginning of the examination of an eye with low acuity whether or not the first of these factors plays an important rôle in the reduced vision. It has long been known that an artificial pupil renders important service in this connection. However an artificial pupil which fits into a spectacle or trial frame is too far removed from the natural pupil to give maximum service. Also we believe the apertures ordinarily used are too large to be of maximum benefit in forming the image. It is truly amazing what an aperture of optimum size and distance from the cornea will do in the way of rendering vision independent of defects in the refractive system.

(2) The uses for an artificial pupil in the laboratory are too well known and too varied to merit noting here. It may be worth while to conjecture, however,

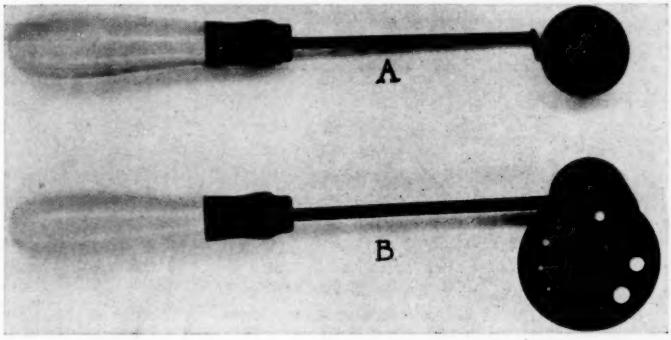


FIG. 1. MODELS OF THE DEVICE FOR CONTROLLING THE ENTRANCE PUPIL
(A) Model with single artificial pupil.
(B) Model with 10 variable pupils which range in breadth from 0.5–6 mm.

that much of the work that has been done with an artificial pupil would have given quite different results had an artificial pupil which more closely approximated the position of the natural pupil been used. Some of it, we have found, can well afford to be repeated under better conditions of use of pupil.

The device to be described has the following features and advantages. (1) When in position the pupil is in almost direct contact with the cornea. (2) It is mounted in an eyepiece which can quickly and easily be fitted to the eye. (3) The eyepiece is mounted on a handle for convenience of placing and holding in front of the eye. (4) Ten sizes of pupil are provided ranging in breadth from 0.5 mm. to 6.0 mm. (5) A change in size can be quickly and conveniently made by rotating a dial, without removing the device from in front of the eye.

A picture of the device is given in Fig. 1. The eyepiece is in the form of a tube 26 mm. in diam., 28 mm. long. The diaphragm forming the front of this eyepiece is slightly concaved so as closely to fit the anterior surface of the eye. At the center of this diaphragm is a circular aperture 12 mm. in diam. Mounted

in a vertical plane directly behind this aperture is the disk containing 10 artificial pupils. It turns about an axle which is parallel to the axis of the eyepiece. This axle is carried by a broad radial arm which is fixedly attached to the tube of the eyepiece by a narrow rim or collar. The tube of the eyepiece telescopes into a second section of tubing or broad collar 20 mm. long, to which the handle of the instrument is fastened. The disk can thus be rotated at will about the axis of the eyepiece. When in position for use, it is turned to the temporal side of the eye examined. In this position, there is no interference with the nose, brow, or other parts of the face.

The 10 pupils are arranged in the form of a circle at a suitable distance from the margin of the disk. They are equally spaced and their centers are at a distance of 16.5 mm. from the center of the axle. Their diameters are respectively 0.5, 1, 1.5, 2, 2.5, 3, 3.5, 4, 5, and 6 mm. When in position in front of the eye, the center of each pupil coincides exactly with the center of the aperture in the diaphragm of the eyepiece. In order that each pupil shall automatically take this position in turn when the disk is rotated, 10 stops are provided. These stops are constructed as follows. The back wall of the disk contains 10 very small, shallow cups, suitably spaced on a circle with a radius of 20 mm., 3.5 mm. greater than the radius of the circle containing the centers of the 10 pupils. In a mount at the center of the radial arm which carries the disk and in exact radial relation with the axis of rotation of the disk and the axis of the tube of the eyepiece, a small ball is sunk against a spring to give tension, at the proper level to engage the cups as they pass in rotation. The positioning of the ball and the spacing of the cups has, of course, to be made with great accuracy and precision, if the center of each pupil is to come to rest at the exact center of the aperture in the diaphragm of the eyepiece. For convenience of rotation the disk is provided with a milled edge, and to permit of smoothness and freedom of movement in turning, the circle containing the cups is grooved. With the provisions made for convenience and accuracy of adjustment, the pupils can be quickly and easily changed either by the patient or the examiner without removing the instrument from in front of the eye.

In order to eliminate reflections and scattered light, the inside of the tube is painted a flat black. All the metal parts of the device are also suitably blackened. Near the farther end of the broad collar around the tube of the eyepiece at a convenient distance from the eye, the handle of the instrument is inserted. This distance is made sufficiently great so that the handle, when held vertically, does not interfere with the cheek or other parts of the face. When wanted for use in a fixed position in a laboratory set-up, the handle can be detached and the tube be held in a suitable mounting as desired.

There is also shown in Fig. 1 a more simple device for the use of an artificial pupil of a selected fixed size. In this case the aperture of the size desired is cut at the center of the concave diaphragm of the eyepiece. In the device shown the aperture is 1.16 mm. in diameter. In a later paper results will be given pertaining to the optimum size of aperture both for emmetropic and ametropic eyes for different intensities of illumination.

A NEW METHOD FOR REGISTERING TIME-INTERVALS

By A. FLEISCH, University of Lausanne, Switzerland

Although the necessity for measuring time-intervals and frequencies arises often in experimental psychology, the methods so far available for this purpose appear primitive and hardly reliable for experiments of a prolonged duration. For many investigations in biology and psychology it would be highly valuable to have an apparatus which should record each time-interval between any two of a long series of phenomena immediately by means of a vertical line. This desired result may be obtained from an apparatus recently described by the writer[1] as a *pulse time recorder*. By its aid it is possible to record the pulse intervals (the time-intervals between two pulse beats) immediately as two successive lines. However, this apparatus may also be used for recording the frequencies of other phenomena and of any other time-intervals.

This apparatus[2] can be used for (a) the continuous registration of pulse-times, *i.e.* the reciprocal value of the pulse frequency, (b) the registering of the duration of respiration and of the duration of inspiration and expiration, (c) experiments connected with the physiology of work, and (d) various uses of the time-recorder.

This pulse time-recorder[3] works as follows. (See Fig. 1.) The rotation of an invariable motor, EM, is transmitted to an axle, W_1, and from this by means of a system of cog-wheels, Z, producing a reduction of speed, to an axle, W_4, to which is attached a rotating magnet, RM. When the magnet is magnetized, the iron disk, S, rotates and winds up the string, Sa, and the slider, Sch, which marks a vertical line on a blackened drum. As soon as the time-interval to be registered is ended by the occurrence of the next phenomenon, *e.g.* the next pulse-beat, the circuit in the rotating magnet, RM, is opened for a short but constant period of time, the slider, Sch, is quickly drawn down by a spring, F_1 and the recording of the next interval sets in immediately. This interruption of the current by the rotating magnet, RM, is effected by means of the pulse magnet, PM, and the steering disk, St.

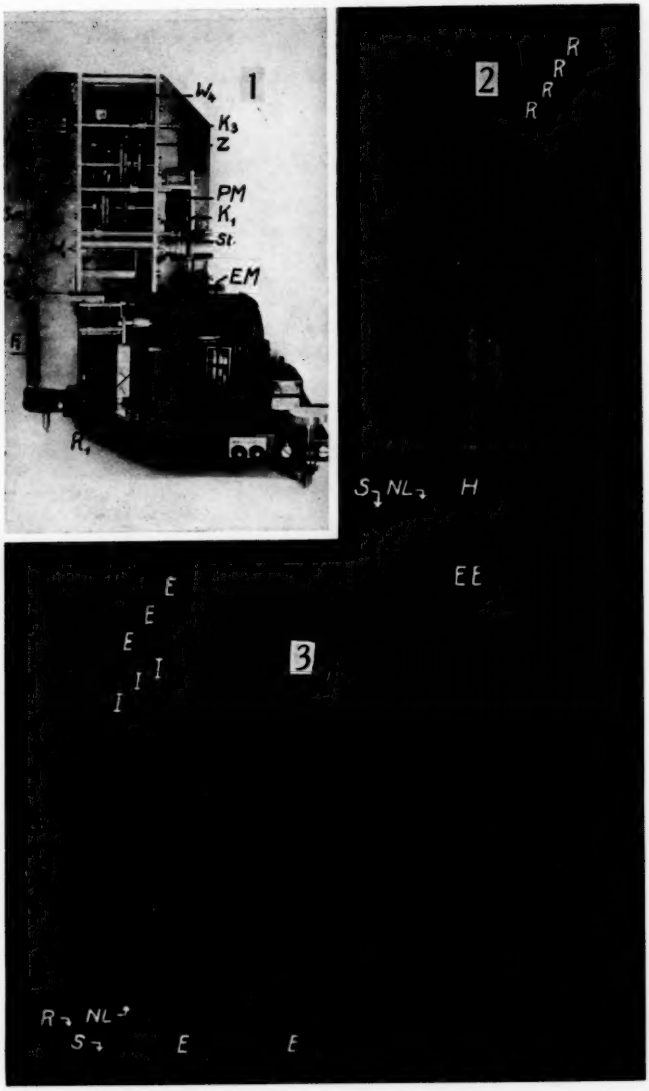
Each time-interval is recorded with a deviation of only $\pm$ 0.004 sec. By a simple shifting of the buttons K_1, K_2, K_3, seven different speeds may be employed with 1 mm. of the recorded line corresponding to 0.01, 0.02, 0.05, 0.1, 0.2, 0.5, or 1.0 sec., respectively. Altogether, this time-recorder may thus register intervals from 0.2 to 138 sec. by means of the corresponding length of the recorded line.

(a) The registration of pulse-intervals. In order to record the intervals between two pulse-beats by means of the apparatus described above, a pulse-pelotte[3] is required which closes the circut for each pulse-beat to cause the pulse magnet, PM

[1] A. Fleisch, Der Pulszeitschreiber: Ein Apparat zur Aufzeichnung der zeitlichen Pulsintervalle als Ordinate, *Zsch. f. exper. Med.*, 72, 1930, 384 ff. See also *idem*, Die Registierung zeitlicher Intervalle direkt als Ordinate mit dem Pulszeitschreiber, Abderhalden's *Handbuch der biologischen Arbeitsmethoden*, Abt. V, Teil 8, 905.

[2] A more detailed description will be found in Abderhalden's *Handbuch, loc. cit.*, which will be published shortly.

[3] The pulse-time recorder and the pulse-pelotte may be obtained from the mechanician of the writer's Physiological Institute.



FIGS. 1-3. See opposite page for legends.

in Fig. 1, to operate. By means of the pulse-pelotte[4] it is easy to record thousands of consecutive pulse-intervals without disturbance, and, given a good pulse, it hardly ever occurs that beats are registered twice or left unrecorded. As the transmission from the pelotte to the pulse-time recorder is effected by means of an electric current, the time-recorder may be placed in a room other than that of the *S*.

The way in which the results of the pulse-time recorder are presented is illustrated by the pulse-interval curve in Fig. 2, which represents the pulse-intervals of a healthy person free from mental or physical strain. Under such conditions the frequency of the pulse is never the same but is subject to constant rhythmic variations. There is always present a respiratory arhythmia of the pulse; synchronously with the respiration the pulse-intervals become either longer or shorter. In addition, certain more extensive variations become visible. At point H, the *S* was put in a pleasant state which at once caused the intervals to become shorter, and increased the frequency. When the excitement was over, the pulse-intervals again assumed their former lengths.

This peculiar arrangement of the pulse-time recorder further registers all extra-systoles of the heart; the shape of the irregularity renders possible conclusions as to the place of origin of the extra-systoles.[5]

(b) The registration of the duration of respiration. By the aid of this pulse-time recorder the length of the individual respirations may be registered directly in the form of an ordinate. For this purpose, one requires an additional contrivance which closes an electric circuit at the very moment of the breathing phase, *i.e.* at the beginning of inspiration. The contrivance used is simple, easy to work and reliable. A belt of an ordinary pneumograph is put around the chest of the *S*, and air is sucked into the pneumograph at the inspiration and expelled at the expiration. These movements of the air in the pneumograph are transferred to a Marey tambour, whose metal recording lever is lowered at the inspiration and by dipping into a vessel of mercury closes the electric circuit. It is necessary that the tambour should be in communication with the atmosphere by means of a T-pipe, so that the air pressure in the tambour and in the pneumograph may always

[4] For a full description, see the references of note 1.

[5] Fleisch, Über das Verhalten des Pulsfrequenz bei seelischer Erregung, registriert mit einen neuen Zeitordinatenschreiber, *Arch. f. Physiol.*, to appear shortly; Fleisch, E. Masing, and R. Beckmann, Die Erkennung von Pulsarrhythmien mittels des Pulszeitschreibers, *Zsch. f. klin. Med.*, 119, 1932, 618 ff.

FIG. 1. PHOTOGRAPH OF THE PULSE-TIME RECORDER. (See opposite page.)

FIG. 2. The time intervals between two pulse beats each have been recorded as vertical lines. The rises of the line are to be measured as from the base line, NL. 1 mm. of the ordinate = 0.01 sec. Time-marks, t = 6 sec., while the signal line, S, curves downwards during H, the *S* is put in a state of happiness. RR represents the rhythmic variations of the pulse times due to respiratory arrhythmia.

FIG. 3. Direct registration of the duration of inspiration, I, and of Expiration, E, as an ordinate by means of the time-recorder. 1 mm. of the ordinate = 0.05 sec. The ordinates to be measured as from the base line, NL. While the signal line, S, curves downwards at E-E, the *S* is in expectation of a disagreeable event. Time-marks, t = 6 sec. The line, R, curves upwards while the duration of expiration is being recorded.

adjust itself to the atmospheric pressure, and that the lever may always return into its normal position.

It is also possible separately to register the duration of inspiration and of expiration. For this purpose a two-armed aluminum lever moving up and down with the breathing like the beam of a balance is attached to the Marey tambour. With the down-movement the ends dip into separate vessels filled with mercury, and thus close the electric circuits required to operate the time recorder. Here, too, a free communication of the Marey tambour with the atmosphere is necessary, so that the beam may not move proportionately to the volume of the air used in breathing, but rather proportionately to the 1st differential quotient of the volume of air, *i.e.* to the speed of the respirated air. With such an arrangement electric contacts are obtained at the beginning of both inspiration and of expiration.

Fig. 3 has been obtained by the method just described. It shows the duration of inspiration and the duration of expiration separately. The longer vertical lines, E, represent the duration of expiration, the lines I the inspiration. In order to avoid any confusion of inspiration with expiration, the signal line, R, provides an additional record of the phases of breathing. The upward movement of R marks expiration, its downward movement denotes inspiration. During E-E the *S* is warned of a disagreeable event; in consequence, the duration of respiration becomes shorter. Furthermore, the relation of the duration of the inspiration to that of the expiration is changed; the relation of these two phases becomes equal instead of 1:1.2 as before.

(c) Application of the pulse-time recorder in the physiology of work. If it is desired to determine a single interval of time, like the time of reaction, a chronoscope may be employed. The latter becomes useless, however, as soon as it is necessary to record, without interruption, a series of time-intervals following immediately one upon the other. For such a purpose, the pulse-time recorder is an ideal apparatus. Thus it may be employed in the physiology of work to record, uninterruptedly, the duration of rhythmic processes of work. When the worker has finished a piece, he puts it down in a special place, thereby closing the electric contact required for the operation of the pulse-time recorder. By this method a complete record of each separate work-interval and of all influences and irregularities is obtained.

(d) Other uses of the pulse-time recorder. The pulse-time recorder makes it possible, among other uses, to conduct serial experiments concerning the precision of reaction, as in the following example. The *S* regards a stop-watch whose fast moving hand completes a turn in 3 sec. On its face the full seconds are clearly marked in red. His task is to close a circuit at the passage of the hand through the second-marks. If it were desired to determine the deviations in the "personal equation" *S* should try to mark the intervals as seen by the eye correctly. If there were no error, all the recorded lines should have a length of exactly 100 mm., as 1 mm. corresponds to 0.01 sec. As, however, the *S* will press the button now too soon and now too late, the recorded lines will be either too long or too short.

It is also possible to conduct by means of the pulse-time recorder serial experiments concerning the time of reaction in a very efficient manner, with each separate time of reaction directly recorded quantitatively.

The pulse-time recorder further may supply, in the simplest manner imaginable,

graphic representations of the highest exactness, when it is desirable to investigate a person's ability to repeat time-intervals, or to divide given time-intervals according to a certain rhythm, as in playing the piano.

Summary. A new apparatus, the pulse-time recorder, is briefly described as recording consecutively, on a blackened drum, the duration of any kind of time interval directly as a vertical line. Several uses of this pulse-time recorder are discussed, as, *e.g.* the registration of the frequency of the pulse, and of the respiration, separate registration of the duration of inspiration and expiration, experiments concerning the physiology of work, determination of the time of reaction, and of the precision of reactions involving time.

A MODEL OF THE SYNTHESIS OF CONDITIONED REFLEXES

By George K. Bennett and Lewis B. Ward, Yale University

In recent months several articles have appeared describing mechanisms which duplicate various adaptive phenomena formerly thought to be peculiar to 'psychical' behavior. Notable among them have been the articles of Stephens,[1] Baernstein and Hull,[2] Walton,[3] and Krueger and Hull;[4] and, from the point of view of mathematical physics, the study of Rashevsky.[5] The appearance of these articles is an indication of the interest in mechanical illustrations of behavior. The present article describes a mechanism employing well-known physico-chemical principles which illustrates several novel and slightly more complex types of adaptive behavior.[6]

Baernstein and Hull used toluene-mercury thermal relays as the basis of their adaptive mechanism. Krueger and Hull produced the mnemonic phenomena by storage of power in polarizable cells. The present authors have attempted a closer approach to the traditional synaptic theory of neural conduction in learning by employing an electro-chemical cell of variable resistance. This type of cell seems to permit of greater flexibility in circuit design, and thus to allow a simulation of higher forms of 'psychical' behavior.

Several types of cell were tried with varying degrees of success, but the one described here was the most satisfactory. It is in essence a lead-acid cell, the polarity of which is dependent on the direction of the previous charge. As used in this mechanism, polarity may be established in either direction, and the cell potential is added to or subtracted from a fixed potential in the circuit to produce or prevent the closure of a relay. This cell differs from the ordinary storage cell, (1)

[1] J. M. Stephens, A mechanical explanation of the law of effect, this Journal, 41, 1929, 422-431; esp. 424.

[2] H. D. Baernstein and C. L. Hull, A mechanical parallel to the conditioned reflex, *Science*, 70, 1929, 14 f.

[3] Albert Walton, Conditioning illustrated by an automatic mechanical device, this Journal, 42, 1930, 110 f.

[4] R. G. Krueger and C. L. Hull, An electro-chemical parallel to the conditioned reflex, *J. Gen. Psychol.*, 5, 1931, 262-269.

[5] N. Rashevsky, Learning as a property of physical systems, *ibid.*, 207-229.

[6] We wish to acknowledge our indebtedness to Professor Clark L. Hull.

in its use in the circuit as a variable resistance, and (2) in that it provides an indiscrete polarization series running from a maximum of one polarity through a zero point to a maximum of the opposite polarity.

In the accompanying diagram, Fig. 1, pushbuttons Su_1, and Su_2 represent the normal, or unconditioned stimuli, for reactions *Ra* and *Rb*, which are lights. Sc_1 and Sc_2 are also pushbuttons and represent stimuli to which the responses *Ra* and *Rb* can be conditioned. The variable resistance cells are represented by the symbols C_1, C_2, C_3, and C_4. As may be seen from the accompanying circuit, *Ra* and *Rb* represent antagonistic reactions in the sense that they cannot occur simultaneously. This is due to the action of the differential relay *Ryd*. Relays Ry_3 and Ry_4

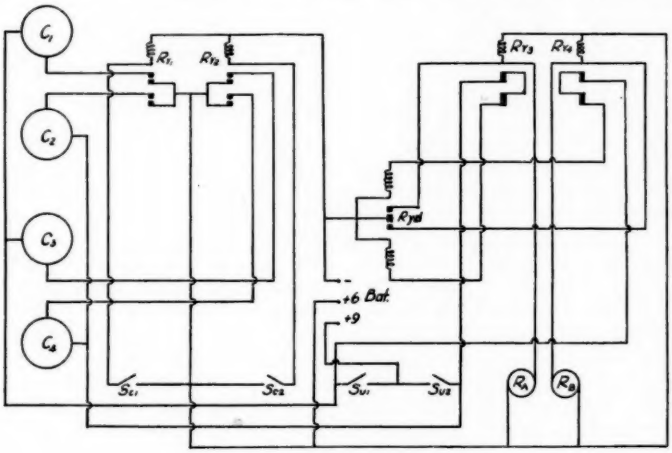


FIG. 1. WIRING DIAGRAM

are arranged so as to prevent the passage of current through the coil of relay *Ryd* which is not in use. The relays Ry_1 and Ry_2 are connected to Sc_1 and Sc_2 respectively. When Sc_1 is pressed, relay Ry_1 connects cells C_1 and C_2 to the intermediate potential source. Relay Ry_3 has a similar function in respect to cells C_3 and C_4, with Sc_2. Cells C_1 and C_3 connect through Relay Ry_4 to one coil of differential relay *Ryd* and through Su_1 (if closed) to the highest source of potential. Likewise cells C_2 and C_4 connect through relay Ry_3 and the other coil of differential relay *Ryd* and Su_2 to the highest potential.

It may be seen that when Sc_1 and Su_1 are simultaneously closed, a potential equal to the difference between the high and intermediate potential values is imposed upon cell C_1 so as to produce a polarity of charge in the same direction as that of the potential supply through relay Ry_1. If, now, Sc_1 is subsequently pressed, the total potential applied to the upper coil of *Ryd* will be sufficient to draw the armature in that direction and produce reaction *Ra*. See Fig. 2 below for a schematic representation.

The differential relay *Ryd* is so built as to be adjustable both as to gap on the

armature contact system and also as to spring tension holding the armature in neutral position. As used in the mechanism described, the differential relay is so adjusted that the flow of current through each of the cells when in a relatively neutral or unconditioned state is insufficient to pull the armature over and cause either response. Thus, when the mechanism is in a neutral or unconditioned state, closure of Sc_1 and Sc_2 produces no reaction.

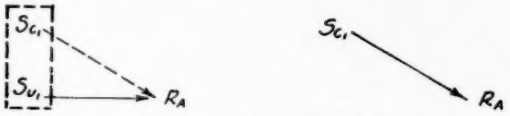


FIG. 2. SIMPLE CONDITIONING

In like manner (as to produce reaction *Ra*) Sc_1 can be caused to produce reaction *Rb*, and Sc_2 to produce either reaction *Ra* or *Rb*. Should Sc_1 and Sc_2 be closed simultaneously, together with Su_1 or Su_2, either *Sc* will produce the reaction corresponding to the *Su* which had been simultaneously closed, illustrating the phenomenon of the conditioning of a single response to multiple stimuli. See Fig. 3 below.

FIG. 3. CONVERGENT SERIES

Should Sc_1 or Sc_2 be closed with Su_1 and Su_2 together, either reaction (but not both) may occur; but on subsequent pressings of the *Sc*, both reactions will occur, one at a time, in a partly random order, depending on the relative strength of the current in the two coils of the differential relay *Ryd*, which in turn depends on the condition of the resistance cells, their previous and present degrees of polarization. Extinction of the so-called conditioned responses to Sc_1 and Sc_2 takes place through the development of a potential in the resistance cells opposite the potential through Sc_1 and Sc_2.

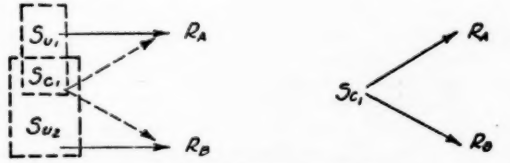


FIG. 4. DIVERGENT RESPONSE

The process described in the paragraph above is that of divergent response, as described by Hull wherein a neutral stimulus is presented successively in conjunction with each of two antagonistic responses (see Fig. 4). When the previously

neutral stimulus is now presented the mechanism has two reaction tendencies which are incompatible. The response which has a stronger tendency will occur; but if it is not reënforced by the appropriate unconditioned stimulus, a successive presentation of the same previously neutral stimulus *may* produce the second response, since the excitatory tendency of the first response has been diminished by its unreënforced occurrence to below the strength of the second response.

It will be noted that the behavior of the mechanism in this case is closely analagous to certain forms of animal behavior usually described as 'trial-and-error learning.' In the case of an animal confronted by a puzzle box situation, if the first reaction is unsuccessful, another is likely to be made, and so on until success occurs or fatigue intervenes. If the animal succeeds in solving the problem

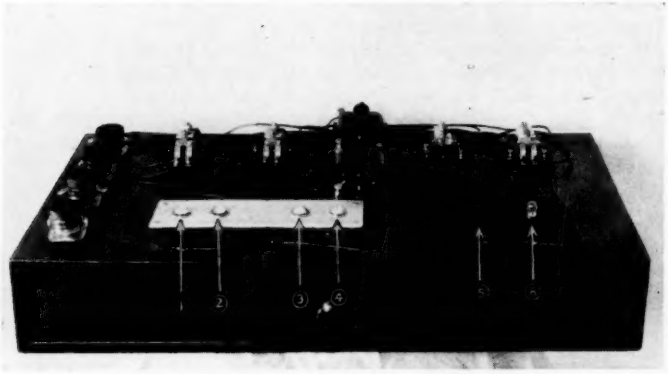


FIG. 5. PHOTOGRAPH OF MODEL

Nos. (1) and (2) indicate Sc_1 and Sc_2 respectively; (3) and (4) Su_1 and Su_2; and (5) and (6) *Ra* and *Rb*. The five relays are seen to the rear, with *Ryd* occupying the center position. The cells are at the left. The box contains six dry cells which are used as a source of potential.

box, the previously successful response will, on subsequent attempts, occur earlier in the series. Since the mechanism described here has only two responses as compared to a very considerable number for even the lowest forms of mammalian life, the trial and error process in the machine is a very much abbreviated and simplified version of its animal prototype.

The apparatus (see Fig. 5) described here illustrates also several of the quantitative aspects of the conditioned reflex. The number of unreënforced occurrences of the conditioned response is dependent upon the frequency of the previous simultaneous presentation, the lapse of time intervening, and the earlier excitatory or inhibitory relationships.

Following these principles there is at present in the process of construction a more elaborate mechanism designed to illustrate phenomena of divergent response on a higher level. It is also hoped to construct, and it does not seem infeasible, mechanisms demonstrating conditioning to patterns, serial conditioning, and possible maze learning.

APPARATUS NOTES

A Device for Automatically Varying a Series

It is sometimes desirable to be able to present a single long stimulus series, or to present half a dozen stimuli in varying orders. On the other hand it may be convenient to register multiple responses on a single recording instrument in some such fashion as Darrow's recording of the galvanic reflex. For such purposes, and in fact wherever rapid and complex switching is desired, the rotary automatic switch will be found useful. The 4 level automatic rotary switch has been used by Burtt for serial reaction time stimuli. However, the combination of a commutator and Strowger 6 level rotary switch,[1] which we have used, has somewhat wider application.

The 4 level switch has been described in detail by Burtt[2] (1931). The 6 level switch is of similar construction. Each level consists of 25 contacts mounted in a semi-circle, while a switch wiper whose axis is at the center of the semi-circle connects each contact in turn. The 6 levels are mounted side by side in a compact unit and all 6 wipers are operated together by an electromagnet. Thus the wipers make 6 connections in each of 25 positions as they rotate through the semi-circle.

We used the switch for automatically varying a series in the following manner. Electrically operated stimulating devices were connected so that one pole of each was attached to a common battery lead and the other to one of the 6 switch levels. The circuits through the 6 wipers and therefore through the stimulators were then closed one after the other by means of a timing device which consisted of a commutator arm running over a series of contacts. Thus the first series was presented. The same 6 stimulators were connected to the 6 levels of the switch in a different order on the second contact. Now when the wipers moved to the second position and were again energized one after the other as before, a second order of stimuli resulted. Similarly a third order was obtained from the third wiper position, and so on around the semi-circle.

An extra contact on the commutator produced the motion of the wipers from one position to another. That is, after the commutator had closed the circuits through the 6 wipers, one by one, it then, by means of the seventh contact, energized the magnet of the switch rotor, and thus caused the bank of wipers to step to the next position on the arc.

We found it convenient to mount the switch and to permanently connect the 6 sets of 25 contacts to Fahnestock clips on a terminal panel.[3] The order of stimuli for each position of the switch wipers, as described above, was then readily procured by connecting the leads from the stimulators in the desired order to the proper terminal board clips. Re-arrangement of the order of presentation merely required altering the connections from the stimulators to the terminal board.

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[1] Switches with as high as 50 points and 3 levels or 25 points and 6 levels are obtainable from the Automatic Electric Co. Chicago, Ill.

[2] H. E. Burtt, Inexpensive apparatus for continuous choice reaction, *J. Gen. Psychol.*, 5, 1931, 286-288.

[3] We are indebted to Mr. Eugene Sayler for advice and aid in mounting the switch.

The switch can be made to step fom one series of 6 to the next automatically, or under key control. It will also return automatically to the beginning of the cycle, even though some of the 25 series of contacts are unused in a given set up. This return is brought about by wiring the stepping magnet to the remaining unused contacts of one of the levels so that as the wipers move to each unused position, they activate their own stepping magnet and move on around the arc to the first position. A single series as long as the total number of contacts (150) may be obtained. Or if desired, two orders of presentation of a series of half this length, three of one third and so on, are obtainable. Two or more such switches can be used in the same hook-up to give a limitless number of combinations.

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A SIMPLIFIED HEAT GRILL

Available heat grills vary in complexity from dual circulating water systems (Dallenbach and Burnett)[1] to interlocking sets of previously chilled and warmed nails (Schlosberg and Carmichael).[2] At the Northwestern Psychological Laboratory we have recently found that variable condensors can be easily adapted for a similar purpose.

These condensors are, in essential, two sets of thin brass plates, one set movable and the other stationary, intermeshed in such a way that the fingers can be placed upon the upper edges of both sets. They can be purchased very reasonably at any radio salvage company and adapted with very little effort for use in laboratory demonstrations of heat experience. The lower portion of the revolving plates is cut away; and the metal rod which connects them and serves as their axis is heated in some fashion. The condensor is set in a pan of ice water so that only the stationary plates become chilled. The movable plates can be warmed either by passing an electric current through them or placing a small candle beneath their axis rod, which projects some distance beyond the frame of the condensor. Since the movable and stationary plates are perfectly insulated, temperature changes in one set can effect those in the other only through air convection (a common feature in all heat grids). The fingers can be placed on the edges of the stationary plates and the movable plates brought in between them so that the skin is stimulated by alternate warm and cold edges. Under these conditions the experience of heat is usually reported.

This apparatus seems to have certain advantages over both types of heat grid mentioned at the beginning of this note. It is less expensive and cumbersome than the dual circulating water system. The space between areas of warm and cold stimulation is much finer than that obtained by use of interlocking nails.

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[1] N. C. Burnett and K. M. Dallenbach. The experience of heat, this JOURNAL, 38, 1927, 418-431.

[2] H. Schlosberg and L. Carmichael. Three new pieces of apparatus from the psychological laboratory of Brown University, *ibid.*, 43, 1931, 119.

NOTES AND DISCUSSIONS

The Meaning of Pleasantness and Unpleasantness

Nafe published in 1924 an experimental report in which he identified pleasantness with a bright pressure and unpleasantness with a dull pressure.[1] Nafe concluded that these pressures *are* pleasantness and unpleasantness. The failure of Young to duplicate Nafe's results, with the exception of one of his subjects who had formerly observed for Nafe,[2] led Nafe to reply that these pressures were not "attention demanding" and would not be reported unless the observer were set for them.[3] This dependence on attitude has recently been confirmed by Wells.[4] A series of investigations conducted by the present writer verified the existence of these pressures and their intimate relation to hedonic tone.[5] The use of the method of correlation, in which a series of pressure judgments were correlated with a series of affective judgments obtained for the same stimuli but at different times, led the writer to conclude, however, that bright and dull pressures *accompany*, rather than *are*, pleasantness and unpleasantness. The fact that correlation establishes a relationship rather than an identity, together with the known dependence of these pressures upon attitude, makes this conservative conclusion quite proper; but one is still faced with the further problem of defining and explaining the relationship in question. We have discovered palpable sensory contents correlating with pleasantness and unpleasantness. These contents are bright and dull pressures; but pleasantness and unpleasantness seem to be something more than these pressures. What they are is the question that this paper attempts to answer.

The situation becomes somewhat clearer if we take a behavioristic approach and regard the words *pleasantness* and *unpleasantness* as conditioned verbal responses to a stimulus-situation, one of whose elements may be the pressures we have been observing. Thus the individual reacts to certain elements of a stimulus situation by giving either of the verbal responses "pleasantness" or "unpleasantness." These verbal responses may be overt, as are usually called for in the experimental setting; or they may be implicit, a mere appearance of the words in the conscious thought processes, as is characteristic in daily life. They may be conditioned to bright and dull pressures or they may be conditioned to other elements in the stimulus-situation. According to this view the terms *pleasantness* and *unpleasantness* have a reference which is not limited to the sensory contents mentioned, but extends to further, non-sensory material as well.

The inclusion of non-sensory material under the affective experience has given rise to the controversy of "meaning vs. process," which can be traced in the hedonic experimentation of the first part of this century. A typical discussion can be found

[1] J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544.
[2] P. T. Young, Studies in affective psychology, *ibid.*, 38, 1927, 175-185.
[3] Nafe, The psychology of felt experience, *ibid.*, 39, 1927, 367-389.
[4] E. F. Wells, The effect of attitude upon feeling, *ibid.*, 42, 1930, 573-580.
[5] W. A. Hunt, The relation of bright and dull pressure to affectivity, *ibid.*, 43, 1931, 87-92.

in the work of Koch.[6] Young recently recognized this ambiguous reference of our hedonic concepts when he said: "The affective judgment is equivocal regarding the existence or non-existence of felt experience. Objects may be judged with little regard to the felt pleasantness and unpleasantness of experience at the time the judgment is made. The evaluation of an object as 'pleasant' or 'unpleasant' tells nothing directly about the felt experience of the subject."[7] In this matter we should not take sides and label affection as sensory or as non-sensory. Both references of the term have been established experimentally time and again. Rather we should understand how the hedonic terms can logically be accepted as referring to both. By thus hypothecating a possible situation wherein pleasantness and unpleasantness, as verbal responses, can become conditioned to more than one source of stimulation, we not only provide a compromise solution of the sensory, non-sensory difficulty, but we bring the findings of Nafe and the present writer on bright and dull pressures into logical relationship with the rest of the field.

Let us start with the assumption that the affective response involves a general reaction of acceptance or rejection of a stimulus situation. These two general types of response are found throughout the animal world. As Piéron phrases it, "Chez tous les animaux, même inférieurs, on voit, à la suite de certaines excitations, des réactions positives marquant l'appetition, l'expansion, la recherche, la poursuite de l'excitant at des réactions négatives indiquant l'aversion, le retrait, la fuite. Nous voyons là l'opposition des réactions affective fondamentales, agréable et désagréable."[8] Troland uses this conception as the basis of his theory, saying that certain harmful stimuli arouse an innate rejection-response and certain beneficial stimuli arouse an innate acceptance-response. This is his principle of nociception and beneception.[9] By conditioning, these responses may spread beyond their original innate bases and become attached to other stimuli. Thus a stimulus may arouse one of two general responses, acceptance or rejection. These responses are of a lower order and are probably set off or mediated somewhere in the lower brain centers, such as the thalamus. It seems reasonable that these responses should involve a general bodily orientation, whose effects in the body-cavity result in the experiencing of bright or dull pressures. So-called pleasant stimuli, for instance, may set off a reflex at the thalamic level, and this reflex involves a bodily adjustment or reaction whose sensory resultants are experienced in consciousness as a *bright pressure*. The conscious reverberations of the bodily orientation to an unpleasant stimulus make up the experience known as a *dull pressure*.

This hypothesis properly implies that the pressure experience is a complex experience of the nature of a perception or blend, rather than a new sensory element depending on specific sense-organs as Beebe-Center would seem to imply.[10] While Nafe's terminology and the attempt to fit these pressures into the Titchenerian touch pyramid has favored this latter view, actual experimentation by the writer would tend to refute it. He found his observers reporting that bright and dull pressures resemble general complexes of sensation, organic perceptions as it were, and was

[6] B. Koch, *Experimentelle Untersuchungen über die elementaren Gefühlsqualitäten*, 1913.
[7] Young, Studies in affective psychology, this JOURNAL, 42, 1930, 20.
[8] H. Piéron, *Psychologie expérimentale*, 1927.
[9] L. T. Troland, *The Fundamentals of Human Motivation*, 1928.
[10] J. G. Beebe-Center, The relation between affectivity and specific processes in sense-organs, *Psychol. Rev.*, 37, 1930, 327-333.

able in a simple analytical situation to get reports of ordinary sensations of pressure in the trunk which correlated extremely highly with 'bright pressure' and 'dull pressure.'[11] This result supports the view that these pressures are the complex sensory resultant of the general bodily adjustment involved in the affective response. In terms of the context theory of meaning, these pressures are the *sensory* core of *affectivity*. Under a sensory attitude of report the presence of these pressures sets off the verbal response of 'pleasantness' or 'unpleasantness.' In such a case the terms *pleasantness* and *unpleasantness* refer to these pressures. They *mean* these pressures. But the meaning of these terms is not limited to bright and dull pressure alone, as sensory experience may or may not be present when a report of "pleasantness" or "unpleasantness" is given.

This difficulty will be cleared up if we examine this 'response' point of view more closely. Originally the verbal response of 'pleasantness' or 'unpleasantness' may have been attached to the sensory experience that we call a bright or a dull pressure; but by conditioning it then may have become directly attached to the stimulus arousing this pressure. This 'short-circuiting' might even result in the stimulus-object's calling forth the verbal response before the appearance of the sensory content which originally gave rise to it. Thus the taste of sugar might call forth the response 'pleasantness' without any necessary intervention of a bright pressure experience. Such a process of conditioning may well explain the tendency of certain observers to 'objectify' hedonic tone or to place it as an attribute of the stimulus-object rather than to look upon it as a subjective experience peculiar to the responding organism. Once we form such a verbal response, it may be used independently on a higher level, *i.e.* in the logical thought processes, without the accompaniment of any sensory determinant. Thus we may speak of pleasantness and unpleasantness without experiencing any sensory content. As Romanes pointed out, "the names of recepts now serve as symbols of the recepts themselves, and so admit of being grouped without reference to the sensuous perceptions out of which they originally sprang."[12]

Unfortunately, the intricate conditionings to which the verbal responses we are discussing may be subjected by the processes of thought do not lend themselves to exact treatment. Such treatment must await the development of our knowledge of the higher mental processes. It is important to note here, however, that in the usual experimental approach to affectivity the observer is called upon to make a report, not of 'pleasant*ness*' or 'unpleasant*ness*,' but of 'pleasant' or 'unpleasant.' The reference of these latter terms is even more equivocal than that of the terms *pleasantness* and *unpleasantness*. Their adjectival nature stresses the objective characteristics of the stimulus rather than the felt experience of the observer. It is only natural that these terms should wander far from any immediate and direct reference to a sensory content of bright or dull pressures.

In closing, it is only fair to mention that the verbal response might become attached to some bodily cue other than these pressures. Such a cue might well arise, as Cannon would have us believe, at the thalamic level, due to mediation of the thalamus in the affective response.[13] These possibilities exist and no doubt may add their perplexing bit to the welter of hedonic experience.

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[11] Hunt, Localization of bright and dull pressure, this JOURNAL, 44, 1932, 308 ff.
[12] G. F. Romanes, *Mental Evolution in Man*, 1889, 75.
[13] W. B. Cannon, Neural organization for emotional expression, *Wittenberg Symposium*, 1928, 257-269.

The interpretation of pleasantness and unpleasantness as conditioned verbal responses, back of which may lie the experience of bright or dull pressures, throws light upon many of the traditional problems in affectivity. Mixed feelings may well occur as verbal responses at a level of logical thought or meaning, or we might have a feeling process of one kind existing at a sensory level and accompanied in consciousness by the verbal response (aroused through another source) associated with its opposite. The argument over the sensory or non-sensory nature of feeling also becomes clear, since bright and dull pressure may explain the sensory side, and the non-sensory side may be explained by the processes of verbalized thought. Finally, the old question of why feeling fades under attention is answered by this view. Attention to the sensory content of pleasantness and unpleasantness, in other words attention to bright and dull pressures, means concentration upon a secondary process, with the attendant removal of attention from the stimulus responsible for the reaction and the consequent fading out of the very content under inspection. Attention to pleasantness and unpleasantness as verbal responses is also fruitless. There is nothing palpable there. Such is the experiential nature of words, and such must be the introspective nature of pleasantness and unpleasantness when these refer to verbal responses. The perfect completion of the hedonic picture must of course await the extension of our knowledge in many fields. In the meantime the view presented here seems to bring some temporary order and unity to the subject.

Dartmouth College WILLIAM A. HUNT

WHAT IS THE CURVE OF FORGETTING?

For some reason or other, errors have frequently been made in textbook accounts of Ebbinghaus' classical experiments on memory.[1] Van Omer and Dallenbach have recently pointed out one of these errors,[2] and we wish here to call attention to another—to the misrepresentation of the curve of forgetting.

Ebbinghaus summarized the results of his experiments on "retention and obliviscence as a function of time" in a table of five columns,[3] which is fully and clearly labelled with headings and sub-headings as follows:

	I	II	III	IV
No.	After X hours	So much of the series learned was retained that in relearning a saving of Q% of the time of original learning was made	P.E.m	The amount forgotten was thus equivalent to v% of the original in terms of time of learning
	X=	Q=		v=
1	0.33	58.2	1	41.8
2	1.	44.2	1	55.8
3	8.8	35.8	1	64.2
4	24.	33.7	1.2	66.3
5	48.	27.8	1.4	72.2
6	6×24	25.4	1.3	74.6
7	31×24	21.1	0.8	78.9

[1] Hermann Ebbinghaus, *Über das Gedächtnis,* 1885; Eng. trans. by H. A. Ruger and C. E. Bussenius, 1913.

[2] E. B. van Ormer and K. M. Dallenbach. A frequent error concerning Ebbinghaus' experiments on obliviscence, this JOURNAL, 43, 1931, 706 f.

[3] *Op. cit.,* Eng. trans., 76.

The figures under column II, *i.e.* the Q%, are calculated from direct experimental results. They express the percentage of saving of time in relearning, *i.e.* the ratio of the difference of time between learning and relearning to the time of learning, hence a measure of retention. The figures under column IV, *i.e.* the v%, are deduced figures by subtracting the corresponding Q% from 100% in order to express the amount forgotten. Ebbinghaus pointed out the significance of these data in the following words:

> One hour after the end of the learning, the forgetting had already progressed so far that one half the amount of the original work had to be expended before the series could be reproduced again; after 8 hours the work to be made up amounted to two thirds of the first effort. Gradually, however, the process became slower so that even for rather long periods the additional loss could be ascertained only with difficulty. After 24 hours about one third was always remembered; after 6 days about one fourth, and after a whole month fully one fifth of the first work persisted in effect. The decrease of this after-effect in the later intervals of time is evidently so slow that it is easy to predict that a complete vanishing of the effect of the first memorization of these series would, if they had been left to themselves, have occurred only after an indefinitely long period of time.[4]

Ebbinghaus discusses here both forgetting and retention, referring sometimes to the figures in column II and sometimes to those in column IV. He represents, however, neither array of figures by graphs.

Ladd and Woodworth were the first to present the data of Ebbinghaus' column II in graphic form,[5] and they, unfortunately for many who followed them, mislabelled this falling curve as the "curve of forgetting."[6] Referring to the figures of column II, those that show the amount retained, Ladd and Woodworth wrote:

> These results may be plotted into a "curve of forgetting" (or of retention), by making the distances along the horizontal axis proportional to the percentages of material retained.[7]

Though it is clear enough from the context that Ladd and Woodworth are discussing retention, they write of the "curve of forgetting," and label the graph representing Ebbinghaus' data on retention as "The Curve of Forgetting."

Following Ladd and Woodworth in time if not in act, Thorndike also labelled the falling curve of retention as the "curve of forgetting."[8] And after Thorndike the error is rife. Pillsbury made it in his *Fundamentals* in 1916,[9] and repeated it in his revised edition in 1922,[10] though he did not make it in his *Essentials* published in 1911.[11] The error is also made by Starch,[12] Watson,[13] Smith and Guthrie,[14]

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[4] *Ibid.*, 76.

[5] Ogden ascribes primary to Ebbinghaus. He says, "Ebbinghaus was able to plot a curve of forgetting which indicates that the impression fades very rapidly during the first hour following the initial memorization" (R. M. Ogden, *Psychology and Education*, 1926, 208). I have been unable to verify Ogden.

[6] G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911. 575.

[7] *Ibid.*, 575 f.

[8] E. L. Thorndike, *Educational Psychology*, II, 1913, 306.

[9] W. B. Pillsbury, *The Fundamentals of Psychology*, 1916, 361.

[10] *Op. cit.*, 2nd ed., 381.

[11] Pillsbury, *The Essentials of Psychology*, 1911, 194 f.

[12] Daniel Starch, *Educational Psychology*, 1919, 156 f.

[13] J. B. Watson, *Psychology from the Standpoint of a Behaviorist*, 1919, 344.

[14] S. Smith and E. R. Guthrie, *General Psychology in Terms of Behavior*, 1921, 111.

Woodworth,[15] Warren,[16] Jones,[17] Gates,[18] Dashiell,[19] Pintner,[20] Colvin, Bagley, and Macdonald,[21] Wheeler,[22] Lucas and Benson,[23] and Monroe, DeVoss, and Regan.[24]

It is a curious and interesting fact that both the origin and reproduction of this error follow closely the course of the error pointed out by Van Ormer and Dallenbach.[25] Trust in secondary sources accounts for the reproduction, but it is more difficult, since Ebbinghaus was explicit in his exposition, to account for the origin. Our purpose, however, is not to explain the error, but merely to point it out so that it will no longer be perpetuated in the psychological literature.

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PARADOXICAL WARMTH

Magnus Blix,[1] while investigating the apparent specificity of sensory nerves, happened upon a demonstration of the punctiform nature of cutaneous sensitivity and, in so doing, wrote the opening paragraphs of one of the strangest chapters in modern experimental psychology. Independently and almost simultaneously with the appearance of his article, Goldscheider,[2] in Germany, and Donaldson,[3] in America, published accounts of their discoveries of the same phenomena. Goldscheider, however, carried his studies further than either of the other two in that he made attempts to determine the effect of stimulating these newly-discovered warm and cold spots with various 'inadequate' stimuli. When he found that the spots reacted to thermal stimuli of opposite sign, he probably regarded this simply as a demonstration that the spots reacted to pressure, since it would have appeared ridiculous to propose at that time that a 'warm' stimulus-object could elicit an experience of cold.

A quotation to be given presently will show that this must have been his attitude on this matter; and the same quotation will make it apparent that Goldscheider actually made the first experimental demonstration of the existence of

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[15] R. S. Woodworth, *Psychology: A Study of Mental Life*, 1921, 360; *Psychology*, 1929, 95.
[16] H. C. Warren, *Elements of Human Psychology*, 1922, 190.
[17] H. E. Jones, Experimental studies of college teaching, *Arch. Psychol.*, 10, 1923, (no. 68), 1-71.
[18] A. I. Gates, *Psychology for Students of Education*, 1923, 259 f.; *Elementary Psychology*, 1925, 331 f.
[19] J. F. Dashiell, *Fundamentals of Objective Psychology*, 1928, 363.
[20] Rudolf Pintner, *Educational Psychology*, 1929, 254.
[21] S. S. Colvin, W. C. Bagley, and M. E. Macdonald, *Human Behavior*, 1929, 233 f.
[22] R. H. Wheeler, *The Science of Psychology*, 1929, 288.
[23] D. B. Lucas and C. E. Benson, *Psychology for Advertisers*, 1930, 261.
[24] W. S. Monroe, J. C. DeVoss, and G. W. Reagan, *Educational Psychology*, 1930, 180.
[25] *Op. cit.*, 706 f.
[1] Magnus Blix, Experimentela bidrag till lösning af fragan om hudnervnas specifika energi, *Upsala Läkäreforenings Förhandlingar*, 1882-1883, 87-102; 427-441. A German translation of this article was published in *Zsch. f. Biol.*, 20, 1884, 141-156.
[2] A Goldscheider, Die specifische Energie der Temperaturnerven, *Monat. f. Pract. Dermatol.*, 1884, 3, (cf. *Gesammelte Abhandlungen*, 1898, 53).
[3] H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 399.

paradoxical warmth and paradoxical cold, although the discovery is commonly credited to Lehmann and the christening of the phenomenon to Von Frey. Be that as it may, paradoxical cold was early recognized as an affair of importance for psychology. It was soon mentioned in various secondary sources and was taken into consideration whenever theories of thermal response were being formulated. As time passed, its acceptance was so ready that it was quickly reduced to the level of an experimental commonplace to be verified anew by successive generations of undergraduates enrolled in the elementary laboratory course.

Paradoxical warmth, on the other hand, never gained this ready recognition; it was completely ignored in the literature and eventually suffered the supreme ignominy of complete obliviscence by its own discoverer. For it is a fact that although Goldscheider plainly described paradoxical warmth in 1884 he proceeded to forget this altogether and to discover it afresh in 1912! Although Rubin made further experimental verification of its existence in the same year, paradoxical warmth again sank to obscurity, failing to merit even passing mention in secondary sources. In view of this state of affairs the writers have deemed it worth while to recover these 'lost' references, to offer further experimental evidence of the existence of the phenomenon, and to insist that any adequate theory of thermal sensitivity must account for its occurrence.

That Goldscheider did indeed make the first experimental demonstration of the existence of paradoxical warmth will be apparent upon careful perusal of the following citation from his paper of 1884:

"Dass es in der Tat lediglich der Druck ist, welcher die specifische Empfindung auslöst geht unzweifelhaft aus der Beobachtung hervor, dass es gelingt, nicht blos mit erwärmter, sondern auch mit stärker erhitzter Nadel mechanisch an manchen Kältepunkten Kälteempfindung hervorzubringen. . . . *Ebenso kann man mit der abgekühlten Nadel, ja mit dem kühlen Messingcylinder an manchen Wärmepunkten Wärmegefühl erzeugen.*"[4]

Even with the quotation out of context it is patent that Goldscheider failed to grasp the systematic significance of the observations he had reported, seeing in them only a demonstration of specificity since the thermal sign of the stimulus was opposed to that of the experience.

In view of this cavalier treatment of his novel findings it is no wonder that Goldscheider failed to remember his own discovery or that twenty years later Thunberg should write:

"Ob es auch eine paradoxe, also eine durch Kältereizung der Wärmenerven entstehende Wärmesensation gibt, ist noch nicht sicher entschieden . . . Gegen die Möglichkeit solch paradoxer Reizbarkeit spricht zwar, dass weder Lehmann noch Alrutz noch Kiesow paradoxe Warmeempfindungen erzielen konnten, obgleich sehr intensive Kältereizmittel (bis —70°) zur Anwendung kamen."[5]

This lack of evidence led Titchener to accept Thunberg's doubt as to the existence of paradoxical warmth and to write in 1910 that:

"Paradoxical sensations of warmth . . . have never been observed in the normal subject. It may be that they do not occur; or it may be that the end-organs are too deeply set in the skin to be reached by a punctiform cold stimulus."[6]

[4] A. Goldscheider, *op. cit.*, 63. (Italics ours.)
[5] T. Thunberg in Nagel's *Handbuch der Physiologie des Menschen*, 1904, 679.
[6] E. B. Titchener, *A Text-Book of Psychology*, 1909, 151.

He notes with Thunberg the existence of a 'perverse warmth' reported by Strümpell in pathological cases but regards it as "doubtful whether such statements do not rest upon a confusion."

This is typical of the situation until 1912 when Goldscheider, with complete disregard for his discovery of three decades previous, proceeds to re-discover paradoxical warmth and to report it as a novel finding. He writes:

"Ich konnte an einzelnen besonders empfindlichen Wärmepunkten mittels umschriebener Kältereize deutliche Wärmeempfindungen erzeugen. Doch gelingt dies nur bei relativ schwachen, nicht bei starken Kältereizen. Am günstigsten schienen mir Reiztemperaturen zwischen 22° and 26° C zu sein. Verschiedene Authoren haben sich mit der Frage der theoretisch zu postulierenden paradoxen Wärmeempfindung beschäftigt ohne das Phanomen auffinden zu können. *Auch mir ist es früher nicht gelungen.* Wahrscheinlich haben wir stets zu grosse Kältereize angewendet."[7]

In the same year Rubin publishes his independent discovery of paradoxical warmth in the following words:

"Ich glaube, dass es mir gelungen ist, entsprechende paradoxe Wärmeempfindungen unter ziemlich normalen Verhältnissen zur konstatieren. . . . Wenn das Wasser im Behälter genügend warm war, bekam die Vp. natürlich . . . eine normale Wärmeempfindung. Wurde das Wasser nun in kleinen Stufen kälter gemacht, dann wurden . . . die Wärmeempfindungen zuerst schwächer, um dann ganz zu verschwinden. Aber wenn das Wasser noch kälter gemacht wurde, kamen mit der jetzt eintretenden Abkühlung der Wärmepunkte die Wärmeempfindungen wieder. Sie wurden zuerst mit wachsenden 'Kältereizen' stärker, um dann wieder abzunehmen und traten bei ganz niedrigen Kältereizen nur sporadisch auf."[8]

Although these two references contain definite evidence for the existence of paradoxical warmth, they have been neglected entirely in the American literature and have received but little better treatment elsewhere. Troland made no mention of them in his comprehensive *Psychophysiology,*[9] although he allowed for paradoxical cold in formulating his theory of thermal receptor action. Fröbes,[10] it is true, does mention the existence of paradoxical warmth but contents himself by citing Rubin and ignoring Goldscheider. The writers have been unable to find any account of the phenomenon in other continental or American sources.

While it is hoped that a more complete and comprehensive program of research on this topic may be presented at a later date, the writers wish to take this opportunity to report certain preliminary observations made with the simplest of laboratory apparatus. The procedure employed was much like that of the common drill experiment on the location of temperature spots. Small grids were stamped on the skin of the ventral side of the forearm and the corners of each square of these grids were then stimulated by means of standard temperature cylinders maintained at fairly constant temperature by frequent immersion in water baths. The routine observed for each subject was as follows:

(1) A map of the warm spots was made with a stimulus-temperature of 39° C.

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[7] A Goldscheider, Revision der Lehre vom Temperatursinn, *Ber.u.d.V.Kong.f.exp. Psychol.,* Berlin, 1912, 227. (Italics ours.)

[8] E. Rubin, Beobachtungen über Temperaturempfindungen, *Zsch. f. Sinnesphysiol.,* 1912, 46, 392.

[9] L. T. Troland, *op. cit.,* 1930, II, 299-312.

[10] J. Fröbes, *Lehrbuch der experimentellen Psychologie,* 1923, 1, 140.

After a few minutes' rest, this map was checked by re-mapping the same area with the same stimulus-temperature.

(2) To prevent *O* from developing a 'set' for reports of warmth the remaining spots were stimulated with a cylinder maintained at 10° C. This led to uniform reports of cold.

(3) The stage was now set for the crucial series. To permit direct comparison between warmth as evoked by a 'normal' stimulus and that elicited by an 'inadequate' stimulus, a cylinder at 39° C. was alternated with one at 26° C. (a temperature reported by Rubin and by Goldscheider as favorable for paradoxical warmth).

The outcome of this procedure may be briefly stated. Paradoxical warmth was reported by 29 out of 33 *O*s, and in no case did the *O* describe this warmth as differing from that obtained by 'adequate' stimulus-temperatures. None of the *O*s suspected that there was anything out of the ordinary since they were blindfolded and the cold cylinder was applied only to the warm spots in the crucial series. *O* thus had either an experience of warmth or no experience of a thermal sort at all. Reports described a variety of intensities for warmths of both sorts, varying from 'weak,' and 'indistinct,' to 'clear cut,' and 'strong.' Frequency of reports of paradoxical warmth for a single grid (2 cm. x 2 cm.) varied from 0 to 8, with a median value of 2. In view of the difficulty untrained *O*s always experience in any attempt to report upon punctiform stimulation of warm spots, these results bulk too large to be dismissed without serious consideration.

The work must be repeated, of course, under more adequately controlled conditions and with trained *O*s before any significant quantitative results can be obtained. In the meantime, however, these results, together with those reported by Goldscheider and by Rubin, should contain an adequate warning to systematists that paradoxical warmth is no longer to be ignored by those who wish to write adequate theories of thermal sensitivity.

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THE VISUAL IMAGERY OF A LIGHTNING CALCULATOR

The performances of Mr. Salo Finkelstein, Polish calculating genius, have recently received favorable attention in the universities of Europe and in the United States. Sándor has made certain quantitative studies of the range of this calculator's mental manipulations, and has furnished some evidence for a comparison with other mathematical prodigies who have been discussed from time to time in the literature.[1] Sándor's observations, in spite of their relative inadequacy, should entitle Mr. Finkelstein to a place among the world's foremost calculators. In addition to the quantitative aspects of the lightning calculator's performances, however, we may study with profit certain significant subjective features. Mitchell, among other things, was concerned with the memory types of mathematical geniuses in so far as they were auditory or visual.[2] Meumann used lightning calculators as examples of his ideational types.[3] This paper involves an extension of these

[1] B. Sándor, Die Gedächtnistätigkeit und Arbeitsweise von Rechenkunstlern, *Charakter*, 1, 1932, 47-50.
[2] F. D. Mitchell, Mathematical prodigies, this JOURNAL, 18, 1907, 61-143.
[3] E. Meumann, *The Psychology of Learning*, 1913, 214-222.

principles of Mitchell and Meumann in that the data are related to the phenomenon of eidetic imagery, and the classification of Mr. Finkelstein as an *Eidetiker*. The results reported are based on about six hours of informal experimentation, and observations made in the course of attendance at three public performances. A number of the times for the various feats have been checked by us with a stop-watch.

The ability involved in Mr. Finkelstein's performance is essentially two-fold. In the first place, he shows an unusual capacity for memorization with respect both to learning and retention. Among his other accomplishments is he able to memorize a visually perceived square of digits, composed of five rows of five digits each, so that the whole series can be repeated in any sequence desired. We have checked this feat on several occasions, and have found him successful at a rate of about 2 secs. per digit. He claims to give offhand the figures of π to 300 decimal places and the logarithm of any number from 1 to 100, to 7 places. Sándor is authority for Mr. Finkelstein's knowledge of the digits of π to 200 places, and there is indirect evidence that the further expansion of this decimal is used in connection with his calculations. His knowledge of the logarithms has been checked in public performances.

In the second place, it is interesting to note Mr. Finkelstein's extraordinary speed in the mental manipulation of figures. He can give the sum of 15 digits at a glance (Sándor), and the speed of addition of 3- and 4-place numbers at least exceeds that of an adding machine. He claims that he could analyze any 4-place number into the sum of four squares, $a^2 + b^2 + c^2 + d^2$. Our observations show that he succeeds in this task usually within one minute and is never over two minutes. All these calculations he performed without extraneous aid; they were entirely subjective.

However, the purpose of this note is not to review what is known about Mr. Finkelstein's speed of calculation, but to discuss the problem of his type of imagery.

The most striking feature of the actual process of calculation and memorization is the normality of the operations. Except for the products, sums, roots, and powers already definitely known, every calculation requires deliberate and fully conscious handling of the figures. He uses short-cut methods whenever possible, but the procedure is always orderly and systematic. The superiority of the processes lies in the fact that he is capable of dealing with large groups of figures. In the task of addition, for example, the average individual is limited to a succession of two and sometimes three-digit manipulations. Mr. Finkelstein, on the other hand, instantaneously perceives the sum of four, five, and six digits, with the result that the sum of a column of figures is given before most people can even read the figures. The superior adult has a memory span of about eight digits dictated in succession. With the same rate of dictation, Mr. Finkelstein is usually able to repeat twenty.

Mr. Finkelstein's process of memorization of numbers is aided by associations. In the course of the experiments, the following varieties were repeatedly reported: (a) dates of historical events; (b) mathematical associations such as powers, roots, logarithms, and prime numbers; (c) permutations of significant numbers; (d) ascending and descending series; (e) telephone numbers; (f) nu-

merical characteristics of literary works, such as number of paragraphs contained. Because of the readiness of the occurrence of associations, if some type of association is not apparent, the numbers appear to be unusual and acquire memory value on account of this lack.

Mr. Finkelstein frequently speaks of "nice" numbers, and the "niceness" is a function of the number of associations aroused. Numbers designated as "nice," however, appear to have an emotional appeal; they are appreciated for their individuality, and automatically arouse aesthetic feelings. The numbers are not endowed with sex, but they possess, for him, uniqueness and personal value. When numbers were written in yellow, a color which he finds unpleasant, he could not avoid a reaction of distinct displeasure. It was, he reported, as if an artistic creation had been hideously caricatured.

Concomitant with the more extensive processes of memorization and calculation, one notes in Mr. Finkelstein a large amount of motor activity such as pacing to and fro, gesticulation, and facial contortion. Before writing the sum of a column of figures or the product of two large numbers written on the blackboard, he invariably draws a line through them. A definite kinesthetic component is here involved. There is a feeling of inhibition when these movements are suppressed.

In respect to the question of visual imagery, it is evident that the imaginal process is virtually integrated into the processes of memorization and calculation, and without it the involved manipulations of figures would undoubtedly be impossible. The imagery may be said to serve a reference function, since numbers resulting from various calculations and numbers which have acquired significance through associations are chalked down, so to speak, and held in readiness for subsequent reference. The imagery, accordingly, leaves the attention free for subsequent calculations, and there is no necessity for continuous review of the figures in order that retention may take place. Certain of the more prominent features of this imagery for numbers are as follows: (a) The numbers appear as if written with chalk on a freshly washed blackboard. (b) The numbers are in Mr. Finkelstein's own handwriting regardless of the form of presentation. (c) Ordinarily the numbers appear to be from 5 to 7 cm. in height. (d) The images normally appear to be at a distance of 35 to 40 cm. from the eyes. (e) The span of imagery includes about six figures with a definite preference for their horizontal arrangement. If, for example, a list of 200 numbers has been memorized, at any one moment any group of about six figures may be made to stand out clearly. (f) When the figures are visualized on a ground at a distance of about 1½ m., they are about 30% smaller and less distinct. Emmert's law of the proportionate variation of the size of the after-image with the distance of the projection ground from the eye seems to be reversed.

The general question of visual imagery necessarily includes a consideration of the so-called after-image. Several tests were undertaken with the purpose of investigating the behavior of this phenomenon. Inasmuch as this calculator manifested a definite color weakness, chromatic attributes could not be reported upon with certainty. The Ishihara test for color blindness revealed a decided deficiency for the four psychological primaries. The general weakness for color may account, in part, for the achromatic nature of the number of images even when the numbers are written with colored crayons.

Successive contrast was tested by having Mr. Finkelstein fixate squares of well saturated color on a medium gray ground for a period of one minute. The image was then projected on a large gray ground which could be moved toward and away from him. Mistakes were made most frequently in the naming of the hue of the after image, and less frequently in naming the presented color. In every case, however, the size of the image varied proportionately with the distance of the projection ground, thus fulfilling Emmert's law. Intermittent appearance of the after-images was, at times, spontaneously reported.

A final test of the effects of color consisted of the presentation of colored figures. Numbers of three digits, each digit about 6 cm. in height, were drawn on white paper. Mr. Finkelstein glanced at the figures, and then observed his imagery of them. Only the four psychologically primary colors were employed. The imagery of the numbers drawn in red, green, and blue behaved in the usual way, that is to say, they were visualized in his own handwriting and were achromatic. In the case of the yellow, however, as has been noted, the situation was quite different. The visualized figures were blurred with yellow, and their form resembled the original figures. Mr. Finkelstein described the experience as both unsatisfactory and decidedly unpleasant. In spite of the color weakness regularly shown, he has a decided preference for certain colors and a decided dislike for others. Yellow is in the group of unpleasant colors.

The high degree of definiteness and stability of the imagery of numbers regularly reported by this calculator necessarily raises the issue of its proper classification. The essential question is whether or not the phenomenon is properly designated as eidetic.

E. R. Jaensch has asserted that the eidetic phenomenon is intermediate between the after-image, AI, and the memory image, MI.[4] Being intermediate, the eidetic image, EI, displays characteristics of both AI and MI. In certain cases there is a preponderance of AI features, and such cases are correlated with the clinical condition designated as "T-type." The preponderance of MI features, however, is correlated with the "B-type."

G. W. Allport contends that the EI is, properly considered, a member of the general class of MI, and differs from the ordinary MI only in degree.[5] The evidence for either one of these views is best tested by examining generally accepted descriptive and functional characteristics of the EI, and relating these to those of the AI on the one hand, and those of the MI on the other. This is essentially what Allport has done. The more certain and prominent of the characteristics listed by Allport[6] may be applied directly to the data which have been collected from Mr. Finkelstein.

(1) Localization. The content of the EI is externally projected and seen. The fulfilling of this criterion is evident in the ready determination of the optimum distance of the projection plane from the calculator's eyes, and in the perceptual character of the imagery which was regularly the writing of chalk on a freshly washed black-board.

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[4] E. R. Jaensch, *Eidetic Imagery,* 1930, pp. 1-73; Über Eidetik and typologische Forschungsmethode, *Zsch. f. Psychol.,* 102, 1927, 35-56.

[5] G. W. Allport, Eidetic imagery, *Brit. J. Psychol.,* 15, 1924, 99-120; The eidetic image and the after-image, this JOURNAL, 40, 1928, 418-425.

[6] Allport, Eidetic imagery, *op. cit.,* 101-113.

(2) Richness in detail (Clearness). In the great majority of cases the EI is superior in this respect to both AI and MI. It has been noted that Mr. Finkelstein's imagery is sufficient to serve a definite reference function. He habitually reads numbers from his imagery.

(3) Persistence. The EI not only persists through long intervals, but may be revived at will by voluntary effort. It was found that Mr. Finkelstein could revive his imagery after long intervals of time. A memorized square of numbers, for example, was repeated by columns two hours after the original learning. This was accomplished by reference to imagery. It may be noted, however, that there exists for him a definite retroactive inhibition, so that the recall of a list of numbers may be inhibited by the memorization of a subsequent list.

(4) Selective tendencies. The capacity for arousing EI is correlated with the interest in the material concerned. The emotional and aesthetic appeal of numbers and their capacity for arousing a wealth of associations could be conditioned by nothing else than a heightened interest. When objects which are relatively uninteresting, such as letters of the alphabet, are visualized, the images are both smaller in size and less distinct.

(5) Flexibility. Details of the EI may be manipulated. Imagined figures are added, subtracted, multiplied, and handled in any way the calculator desires in the course of long and complicated calculations. Furthermore, he is able to call on large groups of imagined figures whenever they are needed.

(6) Invariability. The EI does not follow Emmert's law as does the AI. It has been noted that the imagined figures decreased in size as the projection ground was moved away from the calculator, hence their behavior was the reverse of Emmert's law.

(7) Displacement in space. The EI is subject to spatial displacements such as being turned upside down, and reversed from right to left. This phenomenon was occasionally manifested in the reversal of the order of two memorized numbers. It never occurred, however, when the imagery was reënforced by definite associations.

(8) Conditions of its arousal and disappearance. Allport considers the following points: (a) Time of presentation necessary for arousal, AI>EI>MI. (b) Rigid fixation necessary for arousal, AI>EI>MI. (c) Possibility of voluntary recall, MI>EI>AI. (d) The best projection ground for the EI is in most cases a homogeneous gray corresponding in brightness to retinal gray. (e) The EI may disappear suddenly, gradually, or by piecemeal. Mr. Finkelstein's images of numbers are sharply distinguished from the AI in accordance with these criteria. There is an inevitable difficulty, however, in distinguishing the usual images of numbers from the MI. This calculator cannot satisfactorily make the separation since the accustomed imagery has become spontaneous, and it habitually appears. He was able to obtain the images with or without his eyes closed and upon a gray projection ground. Ordinarily the imagery occurred freely without extraneous aid. No definite manner of disappearance of the imagery was reported.

The application of the eight criteria definitely indicates that Mr. Finkelstein's images of numbers may be classified with certainty as EI. But the data go further in that they show in a striking manner that the essential technical requirements of the EI may be amply fulfilled, and yet the imagery may fail to show any of the salient features of the AI. The analysis lends support to Allport's conception

of the EI as being a special variety of the MI. The difficulty in relating the conditions of arousal and disappearance of the number images to those of the MI must be interpreted as giving validity to this characterization of the EI. Application of the essential criteria indicates a type of imagery that differs from the usual MI only in degree, that is, in vividness, stability, and flexibility. Only one definite feature of the AI has been noted, namely, the external localization. Even here, as Allport has pointed out, the usual *dinghaft* attitude of the *Eidetiker* may account for the external quality of the imagery so that the evidence of the localization of the EI in perceptual space is relatively uncertain.[7]

In summary we may state that the examination of Mr. Finkelstein's images of numbers indicates: (1) the imagery serves a reference function in the mental manipulation of figures; (2) the images are eidetic; and (3) the EI are phenomenally and probably genetically related to the MI, but essentially unrelated to the AI.

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AUDITORY ACTION CURRENTS

In a recent review of the subject of electrical phenomena of the ear, Kreezer summarized the various papers which had appeared on the subject up to July, 1932, and, after pointing out the various pitfalls and physical artifacts encountered in the experiments which were reported, came to the conclusion that on the basis of the available evidence no one had conclusively proved the existence of auditory action currents.[1] We believe, however, that in view of the evidence summarized below, much of which was not then available, that there can no longer be any doubt that such action currents have actually been obtained by most of the workers in this field. The action currents in question have the same characteristics as those recorded from peripheral nerves and from other tracts of the central nervous system, such as the optic or proprioceptive.[2] They meet all the criteria used as tests of true action currents.[3] The doubts and difficulties in positively identifying action currents in the auditory tract, as opposed to other tracts, were largely due to the fact that when a sound is applied to the ear two entirely distinct electrical effects are produced: (1) a disturbance originating in the cochlea and spreading thence through all of the tissues of the head; and (2) true action currents associated with nerve impulses in the acoustic pathways.[4] Of course the action currents can also be detected at a little distance from the nerve itself, with adequate electrodes and amplification, just as the electrocardiogram is recorded from the limbs. The fundamental distinction between the cochlear phenomena and action currents lies in their respective sources.

The cochlear "spread" is most intense as recorded from the round window,[5] although it is also relatively strong in the subarcuate fossa and in the internal auditory meatus. At other points the intensity seems to depend primarily on the distance from the inner ear and the electrical resistance of the intervening tissues. The latency between the arrival of a sound wave in the cochlea and the appear-

[7] According to Jaensch's classification Mr. Finkelstein might be regarded as a 'pure' B-type.

[1] G. Kreezer, this JOURNAL, 44, 1932, 638.
[2] L. J. Saul and H. Davis, *Trans. Neurol. Soc.*, in press.
[3] Saul and Davis, *Trans. Am. Otol. Soc.*, 22, 1932, 137.
[4] Saul and Davis, *Arch. Neurol. and Psychiat.*, 28, 1932, 1104.
[5] E. D. Adrian, *J. Physiol.*, 71, 1931, xxviii.

ance of spread anywhere in the head has not yet proved measurable, but is less than 0.1 σ.[5] The phenomenon is very resistant to narcosis, persists (although with reduced intensity) many minutes after death, and reproduces the frequency of the stimulating sound up to at least 5000 per sec. The cathode ray oscillograph shows that the spread also reproduces the wave-form of the stimulus above 800 per sec., although at lower pitches the response may be more complex.[6]

The action currents, on the other hand, are localized in the auditory tracts, are more intense in the larger tracts and centers than in the 8th nerve (despite the proximity of the latter to the cochlea), show the usual sensitivity to local or general anesthetics, and disappear just before or instantly at death. They follow the frequency of the stimulating waves up to but not over 900 or 1000 per sec. and with higher frequencies of stimulation appear as scattered asynchronous impulses. They travel with the usual speeds of nerve impulses, and appear on the cathode ray oscillograph as waves of shapes and time relations characteristic of action currents in peripheral nerves.

These two effects can be separated and studied independently, the spread by leading off from the round window, where it is very intense and where little or no action-current component is encountered, and the action currents by using highly localizing concentric electrodes,[7] choosing tracts at some distance from the cochlea, and suitably adjusting the intensity of the stimulus and the degree of amplification.

In regard to the possibility that the electrical disturbances might arise from vibration of the electrodes relative to the tissue with resulting changes in the electrical characteristics of the circuit, we may mention one of the numerous controls which we have employed. With electrodes in place in an auditory pathway we touched the handle of a vibrating tuning fork to the clamp supporting the electrodes. This must have caused the electrode to vibrate strongly with respect to the tissue, but only a barely perceptible response was obtained, less than when the fork touched the table or the stethoscope leading to the cat's ear, and obviously referable to stimulation by the clearly audible sound in the room. A movement of the electrode of 2 mm. to a point giving very feeble responses to auditory stimulation abolished even this response.

It is not the intention of this note to go into further details of technique and of controls, and many of the statements in the foregoing outline are to be regarded as first approximations. We wish primarily to point out the chief cause of previous difficulties; namely, the presence of two distinct physiological effects. We are not acquainted with any work on this problem which is in conflict with this interpretation except for the totally negative results of Kreezer and Darge,[8] which we are quite at a loss to explain. The results of the other workers are readily reconciled on the basis of differences of technique, chiefly in degree of amplification used and the location of their electrodes. In literature, as in our personal discussions with Adrian, Wever, and Crowe and his associates, we have found all of the results to be mutually confirmatory and attesting the reality of auditory action currents.

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[6] Unpublished observations.
[7] E. D. Adrian and D. W. Bronk, *J. Physiol.*, 67, 1929, 119.
[8] G. Kreezer and H. Darge, *Science*, 75, 1932, 105.

A Child's Spontaneous Reports of Imagery

At various times my older child, Henry, has made a number of 'observations' spontaneously, indicating that a child five years of age and even earlier not only has, but may even report on, phenomena which are commonly regarded as belonging to the realm of the trained introspectionist. How generally children have reported on these various types of images and sensations, with the exception of so-called eidetic images which have been extensively investigated, I do not know. The fact of interest in the present case is that the reports were given without any previous training, without the child's attention having been directed to them, and without questions or the formality of an investigation. The nature of the reports and the circumstances attending them are as follows.

Negative after-image. While riding in the car with me on a sunny, cloudless day, Henry, then aged 3 yr., 2 mo., remarked as we entered a wooded stretch of road: "I still see the sun when I look at the trees, only now it is dark, much darker than it was." Obviously he had been looking at the sun and the trees furnished a favorable background for the after-image to appear since the rest of the landscape and sky were rather brilliant.

Eidetic image. While passing through one of the rooms of the house when he was about 4 yr. of age, Henry stopped and broke out with the following: "Do you know, daddy, I can see pictures on the wall in here if I look at it and think." I asked him if he could see the pictures outside and he answered in the negative, and upon being asked if he could see them at night (which for him meant after dark, even in summer) he replied: "No, not outside and not when it's dark. Only when it's not too light or too dark; only when it is just right." I have not verified his observation that the eidetic image appears best under medium to shady illumination since the report was made without solicitation and so the fact must be accepted as valid, at least for this child at that particular time.

Dream images. From the time he was a little over three years of age Henry has reported dreams. Since no record has been kept of these reports I shall not try to summarize the nature of the dreams. This must be a common and easy type of report to get with children since my other child, now 3 yr., 5 mo. old, also reports her dreams, but has never duplicated the observations of Henry with respect to any other type of imagery.

Synesthesis. The observations showing synesthetic experiences have been made within the last two weeks, and like the others, have been given without the least suggestion. While engaged in play on the lawn with a number of other children (I was reading on the porch), the name of one of the children "Elsa" was called. Henry spoke up and said: *"That sounds like dark blue."* Upon my trying to question him further concerning other possible color-sound combinations the other children, much amused at his remark, immediately parodied him in mentioning names and colors so I gave up the attempt and said no more. About two weeks later after having been vaccinated (the vaccinating needle having been inserted well under the skin) he remarked as he left the doctor's office: "It hurts still; *a sort of pinkish hurt.*" This was at the age of 5 yr.

After-images of movement. Several months ago Henry made the statement that if things go too fast you don't see them moving at all. In attempting to get him to explain what he meant his exposition made use of the following illustra-

tion: "If you move your hand fast, just like this (suiting the action to the word), you see a whole lot of fingers but some of them don't move." This observation was probably first made as a result of a device used by Henry and his playmates to illustrate speed of movement. A slow motion of a train or animal is shown by moving the hand and with index finger extended, and faster motions with the whole hand thrown across the field of vision. Probably during one of these fast motions he had noticed the multiplication of images which he used to illustrate his assertion concerning fast movement.

Paradoxical heat sensation. At the age of 4 yr., 6 mo., in attempting to reach the faucet, Henry suddenly jerked his hand from under the spigot and wiped it. Thinking he had received a drop of hot water on his hand, I chuckled at his discomfiture. His come-back was: "It felt hot, but I *know* it was cold." All of this happened within a second or two after he reached for the faucet. I believe he knew the stimulus was cold after touching it with the other hand, just as a second, more deliberate touch assures us of the 'real' temperature of a stimulus under similar conditions.

It would be gratuitous to attempt to answer an hypothetical critic's objections that these responses merely represent 'verbal reactions' or special conditioning of some sort. It has not been our custom to discuss synesthesis, negative after-images, and the like at home. Moreover, if we did, I fail to see how a child of his age could grasp what these terms could mean unless he had the experiences corresponding to them. I have purposely refrained from asking questions or making suggestions. As a rule the facts are seldom mentioned again by Henry. They have the appearance, judging from the condition under which they have been reported and the manner of report, of being clear, novel experiences which are mentioned for these reasons. The reports have been in the nature of exclamations similar to those one hears from children the first time they visit the zoo or the circus or get a new toy. The nearest thing to psychological experience of any extended kind which he has had was the Stanford Revision, given after most of these reports were made. That it is not a matter of conditioning appears certain in view of the fact that his sister, as noted above, has not reported experiences which he did at the same age. Nor do I believe it is a matter of intelligence for the other child actually received a rating 11 points higher on the Stanford Revision, given by a member of the Education Department (not the father!).

There is no reason why children should not have these various types of experience at a very early age, possibly soon after birth. It seems to be largely a matter of getting the report under conditions excluding suggestion or any other contaminating influence. I have thought it worth while to record this case because I am certain of the spontaneity of the reports and their freedom from any sort of outside influence.

Bryn Mawr College HARRY HELSON

A NOVEL EXPERIMENT WITH THE NEGATIVE AFTER-IMAGE

Children in India find pleasure in a little pastime they call "Seeing the big white man." The child stands in the sunshine and fixates a small object in his shadow for several minutes. Then he looks at the sky. A very large white image of the child's shadow will be seen against the sky. In the blazing sunlight

of India this after-image is quite clear and distinct and persists for some time. Even small children obtain it readily.

This experiment illustrates in a novel way how the distance of the projection background affects the size of the negative after-image. First year psychology students performing this experiment should never forget the principle of distant projection.

NORMA V. SCHEIDEMANN

June Etta Downey: 1875-1932

With the death of June Etta Downey on October 11, 1932, psychology lost a distinguished contributor and the University of Wyoming one of its outstanding scientists.

Dr. Downey was born on July 13, 1875, in Laramie, Wyoming, the daughter of the founder of the University of Wyoming, one of the state's most distinguished pioneers. She received her B.A. from the University of Wyoming in 1895, her M.A. from the University of Chicago in 1898, and her Ph.D. from Chicago in 1907. With the exception of two trips abroad, and summers at Cornell, Chicago, Iowa, and the Boston Psychopathic Hospital, her entire career was spent at the University of Wyoming. From 1898 to 1902 she was an instructor in English, from 1902 to 1905 an assistant professor, from 1905 to 1915 professor of English and philosophy, and from 1915 to her death professor of philosophy and psychology and head of the combined department. From 1924 to 1926 she was coöperating investigator in the National Research Council migrations research. She was coöperating editor of the *Journal of Applied Psychology,* a fellow of the American Association for the Advancement of Science, in the starred list of *American Men of Science,* a member of the Society for Experimental Psychologists and of the American Psychological Association. From 1923 to 1925 she was on the Council of Directors of the American Psychological Association. She was also a member of Sigma Xi, Phi Kappa Phi, the Colorado-Wyoming Academy of Science and the American College Quill Club.

Her earliest publication, *A musical experiment,* in this JOURNAL in 1897, reflects an interest in aesthetics that was to continue throughout her life. Subsequent publications in aesthetics (except for the study of the effect on a musical program of the familiarity and sequence of selections, published in 1927) were centered in the literary field and were concerned with such topics as: the imaginal reaction to poetry, literary self-projection, emotional poetry and the preference judgment, a program for the psychology of literature, and the psychology of figures of speech. Two books: one, *Plots and Personalities,* written jointly with E. E. Slosson (1922) and another, *Creative Imagination* (1929), also show this interest.

Her interest in the field of sensation and perception is represented by several papers on imagery and synesthesis. An article on three generations of color-blindness was published in 1931.

Dr. Downey's interest, however, lay in motor rather than sensory phenomena. Her Ph.D. thesis, *Control processes in modified handwriting: an experimental study,* done under the direction of James R. Angell and published in the *Psychological Monographs* in 1908, was the first of numerous papers dealing with handwriting, automatic phenomena, muscle reading, the reading and writing of mirror script, writing under distraction, the retention of writing skill after lapse of practice,

handwriting disguise, and pen lapses. Later out of these studies were to come the will-temperament tests with which her name is most prominently identified. From 1910 to 1924 she summarizes the literature on handwriting for the *Psychological Bulletin* under the title *Graphic functions*. From the studies of writing under many varying conditions, she moved on to analyses of family resemblance in handwriting, judgments on the sex of handwriting and judgments of similarity and difference in handwriting. In a book, *Graphology and Psychology of Handwriting* (1919), much of this work is summarized and evaluated in relation to the literature. Her researches on handwriting led to her studies of handedness and dextrality types, a recurrent interest represented by a number of papers. In the period from 1917 to 1920 she published several papers on intelligence tests. This was never a major interest with her, however, except insofar as intelligence is related to personality.

In 1919 her first publication on the will profile appeared, a term she soon changed to *will-temperament*. There followed a number of studies exploring the possibilities of will-temperament testing, studies which culminated in a book, *The Will Temperament and Its Testing* (1924). Here she distinguished three personality types: a fluidic, speedy, hair-trigger type, a dynamic, aggressive, forceful type, and a slow, deliberate, inhibited type. Dr. Downey viewed personality as an integrated whole and objected to abstracting a trait from its setting. Hence she placed emphasis on a plotted profile rather than a single measure. The will-temperament tests attracted much attention and resulted in much research by both Dr. Downey and other workers. Although their subsequent use and evaluation has revealed decided limitations, they may be regarded as pioneer efforts to measure personality and as contributions that have stimulated and carried forward scientific thinking.

Throughout her entire career Dr. Downey was a tireless worker despite limited facilities and a heavy teaching load. Her scientific publications as listed in the *Psychological Register* number 76, of which 2 appeared in her third decade, 18 in the fourth, 31 in the fifth, and 25 in the sixth. Besides she was the author of 29 popular writings including poems, plays, short stories and articles. A volume of her poems was published in 1904. She was also the author of *Alma Mater*, a University of Wyoming song. She also wrote many book reviews both for scientific and popular journals. In 1927 she published *The Kingdom of the Mind*, a book for children and younger readers which presents the results of experimental psychology in popular form.

Her teaching was marked by brilliancy, breadth of interest, fertility of ideas and an unbounding enthusiasm for psychology and its significance for human affairs. Much of this enthusiasm was contributed to her students. Despite her literary, musical, and artistic interests, she consistently emphasized the importance of the laboratory and of a scientific approach to the complexities of human behavior. Always there were interesting things going on in her laboratory, many of which did not find their way into print. Her success in securing the interest and coöperation of undergraduates in psychological research was striking. Unfortunately at Wyoming she had access to but few graduate students.

Personally Dr. Downey was quiet and retiring, somewhat diffident on first acquaintance, but possessing rare charm, an alert, original and stimulating mind, and a kindly and deep human feeling.

University of Minnesota JOHN E. ANDERSON

Edgar James Swift: 1860-1932

Edgar James Swift was born July 24, 1860, at Ravenna, Ohio, received his bachelor's degree from Amherst College in 1886 and for three years, 1889 to 1892, traveled and studied in Europe, spending some time with Wundt at Leipzig and more with Ebbinghaus and Paulsen at Berlin. From 1895 until 1900 he was at Stevens Point Normal School and it was here that he began his psychological and educational publications, eleven original articles appearing during this time.

In 1900 he entered the graduate school at Clark University where, under the direction of G. Stanley Hall, his interest in these subjects was intensified and all the more, perhaps, because he and Professor Hall did not wholly agree upon important conclusions. During his three years at Clark University he published ten more studies upon psychological and educational subjects. It was in Worcester that he met and, in 1906, married Claire Martha Coburn.

In 1908, five years after taking his doctorate, his first complete volume, *Mind in the Making,* appeared. His next two books, *Youth and the Race,* published in 1912 and *Learning by Doing,* 1914, went further into detail in regard to pedagogical method and in his *Psychology and the Days Work,* published in 1918, many of the principles are restated and re-related to each other, other subjects are added and rearrangement makes it obvious that the book was intended primarily for the general public. These books made a deep impression upon those interested in education as Dr. Swift's letters plainly show. Miss Helen Parkhurst, who organized the Dalton School, freely acknowledges her debt to him and the ideas behind the Dalton School have influenced American education very greatly and are still influencing it. The specific modifications introduced with that system have, in other schools, been extended and changed, but Dr. Swift's great contribution to the movement must be acknowledged.

His next book appeared seven years later in 1925. In the interim he had published fifteen original articles, many addressed to the public through the medium of *Scribner's Magazine* and some through trade journals, but there were also such experimental studies as that on the "Perception of Filled and Empty Time." The 1925 book appeared with the title, *Business Power through Psychology,* but it reappeared shortly afterward under the name, *How to Influence Men.* This book frankly deals with the application of psychological principles to modern business and constitutes a very real effort to meet the public upon their own ground, to humanize psychology and to disseminate psychological knowledge in a form available for practical application.

Dr. Swift returned to his original interest long enough to republish his *Youth and the Race* under the title *Psychology of Youth,* 1927, and to complete the subject and bring it up to date with the *Psychology of Childhood,* published in 1930. During this same time he was also preparing a work which was to carry a fight offensively against what he considered ignorance and superstition. Out of this book he got the most fun and perhaps in it he put the most work. *The Jungle of the Mind* was published in 1931. It is as much a philosophy as a psychology; in it he scorns the fine-spun arguments of scientists and philosophers about beginnings and finalities and spends his effort against what he regards as the most common fallacies, the beliefs which most retard the advance of civilization and

which may ultimately defeat it: common credulity, the tendency to build logical systems upon emotional bases, the common belief in magic and the common denial of the universality of natural law. His thesis is that a superficial stratum of elegance cannot successfully be laid over an essentially vulgar attitude toward life.

All of these books were published during Dr. Swift's connection with Washington University for, upon receiving his doctorate from Clark University in 1903, he had accepted an appointment there, and until 1924 he was head of the Department of Psychology and Education. After the creation of separate Departments of Education and Psychology in 1924, he continued as head of the Department of Psychology until his retirement in 1931. He taught summer sessions at Peabody College, 1914; the University of Wyoming, 1915; the University of Chicago, 1916; Kansas Agricultural College, 1917; the University of California, 1921; and he was Special Lecturer of Applied Psychology in the Post Graduate School of the United States Naval Academy at Annapolis from 1921 through 1927. He was a lecturer at the Naval War College at Newport, R.I., from 1921 through 1924; a fellow of the American Association for the Advancement of Science, a member of the American Psychological Association, Western Philosophical Association, Society of College Teachers of Education and Phi Beta Kappa. He died August 30, 1932, at Hollis Center, Maine.

Besides the seven volumes mentioned Professor Swift published, during his life, eighty-four articles and reviews. He wrote and worked earnestly and happily, to carry a message: the necessity of using what knowledge we have. He contributed much to the sanity and wisdom of his generation and presented his subject with strength and dignity but entertainingly, and his homely council was sane, simple, penetrating and practical.

Washington University JOHN PAUL NAFE

Ludwig Reinhold Geissler: 1879-1932

Ludwig Reinhold Geissler, a member of the faculty of Randolph-Macon Women's College since 1920, died on December 15, 1932, after a prolonged illness. Born in Leipzig, Germany, on September 22, 1879, he came to this country in his youth with his parents, who settled in Texas. It was there that he received his early training and education.

After his graduation from the University of Texas in 1905, he transferred to Cornell University for graduate study with Titchener. He received his doctorate from Cornell in 1909, and remained there one year as instructor in psychology. He then served one year as research psychologist at the Nela Park Industrial Laboratory, Cleveland, Ohio; four years as associate professor of psychology and education at the University of Georgia; and four years as assistant professor of psychology and education at Clark College. While at Clark he was also non-resident lecturer at Wellesley College. In 1920, he went to Randolph-Macon Woman's College as Professor of Philosophy and Education and Director of the Psychological Laboratory—the position that he held at the time of his death.

During the early part of his professional life Geissler was concerned with scientific and systematic problems; and his contributions during these years numbered fourteen papers, all of which were published in this JOURNAL. His studies

upon attention[1] were systematically important, as they shaped the Titchenerian doctrine which was formulated during his stay at Cornell.

After his brief sojourn at the Nela Park Laboratories, his dominant interest became technological, and his studies and papers were directed upon the practical aspects of psychology. He was co-founder and co-editor with G. Stanley Hall and John Wallace Baird of the *Journal of Applied Psychology,* and during his editorship, which was discontinued when he left Clark for Randolph-Macon, he was a frequent contributor to its pages. During the latter years of his life, in spite of ill health and remoteness from the industrial centers, he contributed numerous articles upon the application of psychology to industry.

His technological articles were at first theoretical. His systematic training showed itself in his inquiries concerning the nature of applied psychology. As time progressed, however, his work became more practical and specific. From 1923 to the time of his death he was State Mental Examiner of Virginia, and from 1923-1929 he served as Psychological Examiner of the State Colony for Epileptic and Feebleminded.

Geissler was a member of the American Psychological Association and the Southern Society for Philosophy and Psychology, serving as president of the latter in 1929.

K. M. D.

[1] Fluctuations of attention to cutaneous stimuli, this JOURNAL, 18, 1907, 309-321; A critique of Professor Wirth's methods of measurement of attention, *ibid.,* 20, 1909, 120-130; The measurement of attention, *ibid.,* 20, 1909, 473-529; The measurability of attention by Professor Wirth's methods, *ibid.,* 21, 1910, 151-156; Professor Wirth on the experimental analysis of consciousness, *ibid.,* 21, 1910, 489-499; Analysis of consciousness under negative instruction, *ibid.,* 23, 1912, 183-213.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Readings in Experimental Psychology. Edited by WILLARD LEE VALENTINE. Special readings by H. M. Johnson, Willis Carter Beasley, Dael Wolfle, and Helen Morrill Wolfle, and a foreword by Albert Paul Weiss. New York, Harper & Bros., 1931. Pp. xv, 606.

This book is written for the introductory course in psychology. It can be used as a supplementary text in courses consisting mainly of lectures and demonstrations but is better adapted to the needs of laboratory courses in which experimental methods are principally emphasized. It is a compilation of papers representative of recent experimental investigations in psychology. In selecting the papers no thought was given to the historical aspects of the science. They are intended to be a sampling of the work of modern American laboratories rather than a collection of classic experiments.

The compilation has been made to fulfill two needs. In the first place it is a source-book which introduces the elementary student to the original accounts of experiments which are treated "sketchily and at second hand in the formal texts." In the second place the book is intended to teach the student to analyze and present data of an experimental nature. In nearly all of the papers the treatment includes a statement of the problem, a presentation of the data, and a discussion of the results with conclusions. In preparing an original paper for inclusion, the editor's general procedure was to shorten it considerably by reducing detailed descriptions of apparatus and methods, excluding "remote generalizations," and "less direct allusions," and simplifying the language wherever necessary. In practically every paper tables and graphs are used for presenting the data.

Descriptions of apparatus, with supplementary drawings and photographs, are frequent throughout the book. Some of these need amplifying, being too brief for the limited understanding of the elementary student. Descriptions of experimental methods and techniques are usually restricted to the specific topic of the investigation in which they are found. An application of these methods to other possible situations would be of material assistance to the student (in his understanding) as to their significance. The explanation of the use of certain statistical constants in analyzing data is inadequate. The student is not given sufficient information to enable him properly to interpret data presented in the form of standard deviations, probable errors, and the like. Questions and references are appended to most of the papers.

In selecting the particular papers for the compilation the editor has not been prejudiced by any particular kind of analysis. He has very carefully excluded allusions to different schools of psychology and represents psychology as an experimental science rather than a number of "isms." The papers are presented under ten main headings. Experiments on reaction-time are placed first. They include studies of reactions to sound, light and touch with two papers on the personal equation. The

conditioned response is then considered with papers on the pupillary and eyelid reflexes and a discussion of the time interval on conditioning, using the finger withdrawal response. Infant behavior is discussed under the titles: "Sensori-Motor Responses in Infants," and "Activities of Newborn Infants." Under the heading of internal stimuli and their responses are included two studies of hunger (one using human subjects and the other white rats), a study of the sex and hunger drives of the white rate and a paper on the obstruction method of measuring animal drives. Experiments on the differentiation of emotional responses in infants, and the differentiation of emotions by systolic blood pressure, together with a study of changes in pulse, breathing and steadiness under conditions of startled and excited expectancy are classed together under the topic of emotion. The topic of postural responses is divided into four subjects. There are three papers dealing with the subject of attention, two with delayed reactions in animals, one with sleep and two with hypnosis. Five papers are placed under the topic of discrimination. Experiments in audition, vision, and tactual localization are reported together with a discussion on "local signs as orientation tendencies." The topic of learning has been divided into two parts. In the first are found papers on the following subjects: animal drives in relation to learning, the mirror drawing experiment, a comparison of human adults and white rats in maze learning, the nature and elimination of unsuccessful acts, brain injury in relation to learning in the white rat, the learning curve for the complex process of generalizing abstractions, the relation between learning and the amount to be learned, and the relation between teaching and learning. The second set of papers deals with complex types of learning. There is one experiment on the multiple choice method as used with primates and three experiments on the evolution of concepts and the relative efficiency of different types of concepts. A paper on the experimental approach to the study of language is the only one to be included under this topic. The last four papers are classified under the heading "Social Studies." The first is a study of the relation between intelligence test scores and scholastic success; the next is an experiment on the use of photographs in predicting success and vocational aptitude; the third is a study of common annoyances and the fourth is a description of a method for measuring public opinion.

Although the writings of a great many individuals are represented in the book, the editor has been very successful in keeping the difficulty of the reading within the range of the elementary student. In only one instance has he failed to achieve this and that is in the experimental articles on vision and audition. A large amount of the content of these papers is sufficiently difficult for advanced courses in experimental psychology.

University of California C. W. BROWN

Handbook of Child Psychology. By JOHN E. ANDERSON and others. Ed. by Carl Murchison. Worcester, Clark Univ. Press, 1931. Pp. xii, 711.

The *Handbook of Child Psychology* represents a series of papers by the twenty-two authors, most of whom are well known for their research work in the fields which they discuss. The editor has apparently made no attempt to restrict the freedom of the authors in any way except by assignment of topics. Probably the most successful papers from the point of view of usefulness to those for whom the book will be of most service (advanced students, teachers of child psychology, and re-

search workers), are those papers which critically analyze most of the important work in the particular field discussed. Such critical analysis involves the presentation of investigations and experiments in such a way that the reader has available information regarding (1) the problem which each investigator or experimenter was attacking, (2) the techniques or methods used in the experiment or investigation, (3) the results, and (4) an evaluation of the extent to which the reviewer finds the techniques and results adequate for the problem attacked. This method of review gives the reader not only a body of integrated information, but also suggests a variety of new problems, and methods to be used or avoided. Authors who have effectively used this method in the *Handbook* and the titles of their papers are: Bühler, "The Social Behavior of the Child;" Gesell, "The Developmental Psychology of Twins;" Goodenough, "Children's Drawings;" H. E. Jones, "Birth Order in Relation to the Development of the Child;" M. C. Jones, "The Conditioning of Children's Emotions;" Vernon Jones, "Children's Morals;" Klüver, "The Eidetic Child;" McCarthy, "Language Development;" and Peterson, "Learning in Children."

Two valuable chapters, the contents of which preclude the type of treatment mentioned above, are those by Anderson and by Lewin. Anderson discusses the history of child psychology briefly and describes methods or techniques which have been used, illustrating with typical experiments. Lewin's paper, "Environmental Forces in Child Behavior and Development," presents a well worked out "system" of child psychology and is a challenge to current theory and method.

Woolley discusses problems of eating, sleeping and elimination, at some length, drawing largely from empirical evidence. Hers is a discussion which should be of considerable value to individuals concerned with the training of infants in these biological functions. Another topic, the inclusion of which in the *Handbook* may be pushing the boundaries of subject matter too far afield, but which makes interesting reading, is Isaacs' description of a super-nursery school or kindergarten. However, there would probably be no exact agreement among child psychologists about what topics should be allotted to a handbook on child psychology and about the relative amount of space that should be devoted to them.

Several omissions seem apparent to the reviewer. The extremes of the distribution of intelligence are discussed in papers by Terman (The Gifted Child) and by Pintner (The Feebleminded), but there is no space allotted to intelligence testing *per se*. This would seem a real omission, especially in view of the comparatively recent but great amount of work which has been done in testing infants and preschool children. Another topic practically untouched in the *Handbook* is that of clinical psychology, *i.e.* with reference to the type of work being done in the many child guidance clinics throughout the country. Several papers skirt the edges of this subject, namely, Anna Freud's "Psychoanalysis of the Child," and Kimmins', "Children's Dreams," but nowhere is there a discussion of the problems attacked by clinical psychology, the methods and results obtained. Such a chapter would be of more practical value than a discussion of the hypotheses of one school of psychoanalysis or of a statistical analysis of the dreams reported by 5000 school children.

At the close of each chapter there is a list of the references used by the authors. These total to some 1300 to 1400 titles. In addition several of the authors have made available additional bibliography. The volume is very satisfactorily indexed with both name and subject indices.

University of California CAROLINE MCCANN TRYON

Genius and Creative Intelligence. By NATHANIEL D. METRON HIRSCH. Cambridge, Mass., Sci-Art Publishers, 1931. Pp. 339.

This book is ambitiously labeled: "A Psychological Foundation for Twentieth Century Political and Sociological Theory and Action" (fly leaf). The main arguments are in favor of the theses: (1) that, in the study of man, the most important undertaking is the study of genius; and (2) that genius is scarcely human (preface).

The author contends that the genius is the greatest single factor of social destiny (preface), though adequate demonstration of such a causal relationship is difficult to see from the data presented. The genius is pictured as of delicate health in early life and often in maturity (p. 318)—(no data are given for the generality!); as precocious mentally and arising to full maturity much later than the average (p. 318); as neither psychotic nor neurotic, yet prone to minor mental disorders (p. 319); as a poor writer, because of a lack of nervous balance, and as lacking in motor coördination (p. 319)—(no comparison is made with non-genius and there is no evaluation of practice and training); as lacking in procreative power (p. 320)—(the insinuation is that he is undersexed—one is led to wonder whether the literature on the sex life of so many men of genius is all wrong); as bashful in childhood (p. 321); as over sincere (p. 322); and melancholic (p. 322). Genius seeks solitude as a defense against the mob (p. 325); has intense faith in himself and his mission (p. 328); and is a biological mutant (p. 320). Statistical analysis forms no part of the base for most of these conclusions. "The genius differs in *kind* from the species, man. Genius can be defined only in terms of its own unique mental and temperamental processes, traits, qualities and products. Genius is another psycho-biological species, differing as much from man, in his mental and temperamental processes, as man differs from the ape" (p. 298).

Unfortunately the term *genius* is never concisely defined by the author. "His origin is biological. Great men are not made by environment, potential great men are often destroyed by it" (p. 38). But just what constitutes genius, what the term should stand for, is not stated. That social appraisal, at some time or other, is essential to the detection of genius is scarcely recognized by the author and that the criteria of evaluation may not be constant for differing social structures seems not to have been considered.

Part I is an introduction to the nature of man—brief in its treatment, but positive in its conclusions. Frequent recourse is had to the teachings of William McDougall—*elan vital* and all! The importance of production and protection of genius is stressed. Since genius, in the opinion of the author, is a mutant, why concern ourselves with its production? So far as the reviewer is aware, all attempts to predict the nature of mutations have been unsuccessful. Dangers of over-populating are feared. Because those who live to be 65, 70, 75, or 80 today do not survive as long thereafter, on the average, as those who lived to those ages a century ago did (when only the very hardy attained those ages at all), it is concluded that a man who lives to those ages today may not expect to live as long as he would had he lived a century ago (p. 51). The differential birth rate is noted and decried (p. 70). Conclusions are drawn from data covering, at best, a 30-to-50-yr. period—certainly a short period of time in which to determine what birth-control information will do for a population. Contraceptive information may reasonably be expected, and the reviewer believes, be shown to become the property of the more fortunate classes

socially, intellectually and financially first, and from them spread downward. If this is so, no one can predict the effects of that information as it becomes knowledge to those considered of lesser social value.

In Part II intelligence is said to have three dimensions: (1) the cognitive aspect of instinct; (2) a sensori-motor, perceptual and reproductive intelligence; and (3) a symbolic, analytical, verbal and conceptual type. Throughout this discussion there is much with which most psychologists can agree and a great deal with which many will find fault. Qualitative differences are made distinct from quantitative ones (a distinction undefined and difficult to follow), and are made an argument against the single-factor theory of general intelligence (p. 111). Inadequacies of present tests and testing methods are emphasized. The conclusion seems to be that since certain improvements have not been made they cannot be made. It is claimed that non-verbal tests do not differentiate above the 9- or 10-yr. level—a conclusion scarcely in keeping with the findings of Edward B. Greene at the University of Michigan (Non-Language Series, Ann Arbor, Edwards Bros.).

Too many things are taken for granted through the discussion of the book. The reviewer is somewhat taken aback by the unsupported statement that "beauty, with intelligence, character, and initiative is declining" (p. 80); that puritan New England had high morals and intellectual poverty (p. 127)—(what are morals anyway?); that ability to use one's native language is unaffected by environmental factors, but ability to use a foreign language is affected (p. 141); that the mouse possesses an instinct of fear (p. 117); that man created the intellectual gap between himself and the other animals (p. 171); that sex is man's greatest frailty (p. 79); and so on *ad infinitum*. This book is altogether too ambitious for the data presented, or, for that matter, available.

Western Kentucky State Teachers College MILTON B. JENSEN

A Personnel Study of 10,000 Iowa High School Seniors. By JOSEPH RAYMOND GERBERICH. University of Iowa Studies in Education, 5, 1930, (no. 3), 1-117.

This monograph is a report of the results of the Iowa High School Survey inaugurated in 1923 primarily for the purpose of securing data for use in educational guidance. Two tests, the Iowa High School Content Examination and the Iowa Comprehension Test, were given during all of the 6 years covered by the study. The Thorndike Intelligence examination given during the first 3 years was replaced thereafter by parts of the Iowa Placement Examination series. The reliabilities of the tests employed vary from 0.76 to 0.97. The results of 6 years of the survey are presented, with ample explanations and discussions, in 50 tables.

Large differences in the test performance of various schools are attributed to probable differences in educational efficiency and the eastern section of the state is compared with the western much to the advantage of the former. That the performance of the larger schools, more prevalent in the eastern section, markedly surpasses that of the smaller schools, more frequent in the western section, is held not to invalidate the sectional comparison inasmuch as the east has more fairly large cities than the west. The reviewer's conception of a fair regional comparison would be one based on schools of the same type in each region. Probably the same differences that were found between the performance of eastern and of western schools would have appeared in a similar comparison of large with small schools in either

section. Again it is quite possible that small rural schools, even if more efficient educationally than large city schools, might still fall lower in test performance because of inherent differences in mentality.

Sex differences in performance on individual tests favor the girls in all the English tests and the boys in all others. These differences are statistically significant in 16 of the 21 comparisons, 12 of these 16 being in favor of the boys.

Correlations between age and the various tests and composites ranged from —0.13 to —0.49, the relationship being only slightly higher in the case of general ability tests than of achievement tests generally. From this fact the author concludes that the older less able students receive more than their rightful share of attention at the expense of the younger more capable ones. However, this fact may as well be explained on the supposition that selective elimination of the less studious and less highly motivated dull students leaves only the more scholarly members of the duller group in school at the senior level whereas no such selective factor is operative among those of high intelligence.

The amount of selection between high school graduation and college entrance is indicated by the following figures which show the number who entered college from each "decile" (author's terminology) of the survey test group beginning with the lowest tenth and advancing to the highest: 10, 17, 20, 31, 26, 20, 35, 34, 44, 79. The median percentile of those who entered the University of Iowa as freshmen was 69.2; for those who entered the second and the third year, respectively, it was 74.9 and 78.1. No sensible gains occurred beyond this point.

In view of the high correlations (0.65 to 0.75) between first semester grade averages and scores on the Iowa Placement Examinations reported from previous studies it is a little disappointing to find that the replacement of the Thorndike Intelligence Examination by the Iowa English Aptitude and English Training examinations, raised the multiple correlation coefficient only from 0.563 to 0.586.

A bibliography of 156 titles is one of the many useful features of this interesting monograph.

Kansas State College JOHN C. PETERSON

Growing Up in New Guinea: A Comparative Study of Primitive Education. By MARGARET MEAD. New York, William Morrow, 1930. Pp. 372.

A six month's trip to the Admiralty Islands was undertaken with the intention of collecting data for three contributions to Anthropology: first, the book under review; secondly, an article on the relationship between spontaneous animism and the thinking characteristics of mentally immature persons (refererred to on p. 289 of the "New Guinea" book); and thirdly, an essay by Dr. Fortune, the author's husband, on the ethnology of the Manus (p. 293). The last two of these seem to be the more serious studies, whereas the first was written in a more popular style, perhaps not without an eye on the financing of future field trips. Although the book possesses certain pretensions to detailed anthropology, its general argumentative and impressionistic manner keep it from demanding extensive attention in a psychological journal.

Mention, however, should be made of two of the author's general attitudes towards her material. In the present book, she has done for the Manus what she did in her

previous book for the Samoans in order to carry further her comparative study of various cultures. This was done in the hope that such study would yield more exact information about what may be considered as inherent and what purely cultural in the nature of man. Properly used, the comparative method is unquestionably one of the most profitable for anthropology, but in practice it is also one of the most dangerous, since there is the omnipresent possibility of identifying aspects which are only superficially similar. In reading Miss Mead's long comparisons of the Manus culture with our own, one feels that she has not avoided this danger.

Secondly, her chief working hypothesis, implied and stated at various points throughout the book (cf. p. 279), is open to some question. This is that a primitive society presents such a unified and simple picture that even a total outsider may go into the community and within six months completely understand it. On this point, there is justification for the most profound disagreement. Even if it be granted that a primitive culture is more simple than ours—a point which men like Rivers, Radin, and Leroy might be expected to deny—the mere understanding and evaluating of a culture other than one's own presents such grave difficulties that it is surprising to find such a view definitely stated by an investigator with as much insight as Miss Mead (p. 5). If anthropological field-work could extract so readily the essence of a foreign culture, it is quite amazing to find trained investigators mistakenly maintaining for sixty years that there was a mysterious animistic tendency inherent in primitive thinking, instead of promptly realizing, as does Miss Mead, that these manifestations could be explained simply on the ground of external cultural factors. That a "trained" field-worker may glibly analyse social customs and fit them into such categories as his training has given him would be readily admitted, but this superficial anthropology is not the complete understanding which should be aimed at—and in fact achieved if anthropology is ever to solve its problems. The mere collection of data under preconceived headings may delay this understanding when those headings are ill-conceived, since it permits the formulation of theories, apparently valid because based on a tremendous number of "facts," but actually quite false, since the facts in question come rather from the reaction of the field-worker to a strange culture than from an adequate interpretation of the essence of that culture.

Cornell University R. S. HILL

The Laws of Feeling. By F. PAULHAN. Translated by C. K. Ogden. New York, Harcourt, Brace & Co., 1930. Pp. xiv, 213.

This is an essay which first appeared in the *Rev. Philos.* in 1884. After four revisions it now appears in translation together with a supplementary essay on Feeling, Intelligence, and Will (*Rev. Philos.*, 1920). Two arguments prepare the way for the general law of feeling, viz., (1) that man is an ensemble of organs united and imperfectly harmonized by one of the organs, the nervous system, and (2) that psychological study is a way of studying the functions of the brain. All affective phenomena, from the most ordinary emotions to the highest and most complex feelings, are the result of arrested tendencies to action, that is, of "more or less complicated reflex action which cannot terminate as it would if the organization of the phenomena were complete" (p. 17). The general law of feeling then is: "An

affect is the expression of a more or less profound disturbance of the organism, due to the fact that a relatively considerable quantity of nervous energy is released without being able to be used in a systematic manner. An arrest of the tendencies aroused and a number of physical or psychical phenomena of various kinds, are then produced. At the same time, one or several or all of the following phenomena always appear: persistence of tendencies, relative lack of coördination and sudden appearance of the phenomena produced, and the tendency of the awakened impulse to monopolize the field of consciousness" (p. 57). This general law is then applied to the production of three classes of affective phenomena, viz., (1) passions, feelings, affective impulses and signs, (2) affective sensations, and (3) emotions. In a discussion of compound affective phenomena, Paulhan almost casts his vote in favor of mixed feelings. A short conclusion relates the study of emotion and the principle of arrested tendencies to the general problem of psychology.

The burden of the supplementary essay is to be found in the argument that feeling, intelligence, and will are not separate facts and, much less, products of three independent faculties. On the contrary, "there is no fragment of mental life, however insignificant, which does not exhibit all three of them at work," their separation being due only to the fact that we are impressed more strongly now by one and now by another of them (p. 204).

University of Illinois COLEMAN R. GRIFFITH

Mental Tests and Heredity, including a Survey of Non-Verbal Tests. By BARBARA SCHIEFFELIN and GLADYS SCHWESINGER. New York, Galton Publishing Co., Inc., 1930. Pp. ix, 298.

This book has two purposes. The first and more general one is to outline the problems involved when mental tests are used for the purposes of eugenics. The second is to survey as completely as possible the non-verbal tests which have been offered at various times for use in appraising "general intelligence." The first part of the book (18 pages) is devoted to the historical attempts at measuring individual differences and to definitions and theories. Part II outlines analytic studies of environmental factors and investigations of mental inheritance in their various settings and approaches. Part III, the largest division of the book (175 pages), is devoted to opinions and experimental data on different sorts of tests, especially verbal and non-verbal, whether for individual or group application; the interpretation of statistical data; and an annoted survey of tests available for the purpose of the authors, as stated. Verbal tests which may be used practically as non-verbal or may be given in different languages without the necessity of re-standardization are included. These tests are classified under the heads of learning; memory; sensory; sorting; special ability and mechanical puzzles; constructive ability; picture; construction; formboards; mazes; recognition of form, color or weight; spatial relations; and group tests. Under each specific test are data on origin, materials needed, directions, responses to score, standardization, reliability and validity, discussion and references. The information consists in quotations from articles and books, and also from personal letters received by the authors. Drawings of materials are occasionally given. The last part of the book summarizes, draws conclusions and makes recommendations; again these are given largely in the form of quotations or summarized statements from the testers themselves. The work seems to have been done with much

care, although there are of course minor errors here and there, and mere quotations often do not represent an author correctly. In such cases the personal letters are helpful.

Useful material and suggestions are found in the appendices. In the entire book different views are assembled not to exhibit disagreements, but to present the picture of the developments and the status of tests as they actually are. The book will doubtless be found a useful tool in the hands of professional psychologists as well as a guide for special workers in the field of eugenics. This will be evident when one considers the origin of the work. It was projected by the Eugenics Research Association, a committee of which—including Clark Wissler, C. B. Davenport, H. H. Laughlin, Charles G. Campbell and Frederick Osborn—outlined the original project and turned over the details of the work to a committee of the Carnegie Institution of Washington under whose auspices the authors worked. This committee consisted of A. V. Kidder, C. C. Brigham, C. B. Davenport, Leslie C. Dunn, H. H. Laughlin, Rudolph Pintner, E. L. Thorndike, and Clark Wissler.

J. P.

The Mind in Action: A Study of Motives and Values. By A. CAMPBELL GARNETT. New York, D. Appleton & Co., 1932. Pp. xii, 226.

Despite the contradiction between the wrapper and the title page of this book, the psychologist soon discovers that the author is really a philosopher. He has taken viewpoints of Ward and Stout, James' self, McDougall's instincts, Spearman's cognition, a modicum of *Gestalt,* some interesting ideas of his own, and developed them into a verbally logical system of sources of motivation and value.

Innate motivation is in terms of need for activity, pleasure and seeking, pain and avoiding, appetites for food, air, sex, and so on—thirteen "instincts pure and simple" (p. 112), and three instincts which are also appetites. The latter half of the book is concerned with acquired sources of action including habits, the self, sentiments of love, loyalty, respect and hatred; and abstract ideas or higher values of beauty, morality, truth, and religion.

Many formal definitions are enunciated. Instinctive behavior is defined as "the pursuit, with more or less foresight, of an innately determined end in response to the cognition of a meaningful situation" (p. 22). Cognition is involved in all instinctive behavior; the latter is always more or less intelligent. Appetites are innate wants which recur periodically in connection with the normal functioning of the organism. Both instincts and appetites are dependent upon structures in the organism and they are essentially the same except that in the case of instinct the structure determines a type of response to a type of extraorganic stimulus, whereas in that of appetite the structure determines a type of intraorganic stimulus. This seems to the reviewer to be a valuable distinction.

"Motor habit is the acquired tendency to respond with certain movements to a certain type of conation" (p. 120). It is of secondary motivational value, being static relative to conative habit. "Conative habits are instinctive responses to the acquired meaning of objects and situations" (p. 120). Begging by a dog when hungry is given as an instance of the former, while attacking by a dog whenever he sees a cat illustrates the latter.

The self with its expansions and appropriations is made to play a very large part in the development of the sentiments and the so-called higher values. Sentiments

are produced through the acquirement of meanings by the self or subject. Values are developed through efforts to develop and maintain systems of relations. The 'self' or finite center of experience plays its part by "continually enlarging its cognitive and conative grasp or penetration of the reality in which it is immersed" (p. 211).

The paragraphs above aim to suggest how the text, as it proceeds from beginning to end, gets increasingly philosophical. Thus an author might start out with the best of intentions but the weight of his major interest might become progressively effective and so produce a gradient in his writing. The actual treatment is 'arm-chair' almost throughout and to the experimentalist reader it is extremely uninteresting. There is a theoretical integration but it contains no new facts. Explanations are in such general terms as conation, cognition, self; adduced evidence is at the anecdotal level. Though a jig-saw puzzle has been completed, too many of the pieces are imaginary. One feels that for the writer the words are sometimes realities and that faculty psychology is breathing. The most serious difficulty of all is the fact that this work, like so many others, stands almost completely apart from specific present-day experimental results.

The book makes little significant contribution to scientific psychology, and definitely it seems not to belong in a contemporary library of psychology with a dual criterion of popularity and scientific accuracy. The systematist and the self-psychologist, however, may study its organization with interest.

Yale University S. M. NEWHALL

Religious Realism. A series of fifteen articles contributed by various contemporary theologians and philosophers. Edited by D. C. MACINTOSH. New York, The Macmillan Co., 1931. Pp. viii, 502.

This book is a decidedly important presentation of contemporary opinion in regard to the fundamentals of philosophy and religion. It is also a possible starting point of new and significant discussion, new methods of approach, and even of possible experimentation, in connection with some of its problems. The articles are perhaps the kind of thing one might hope to get should the American Association of the Advancement of Science add a section on theology to one of its divisions. The greatest variety and freedom are exercised by the various authors, both in their choice of topics and manner of treatment, yet the book succeeds in its endeavor to present a common, objective, point of view, and it is properly entitled "Religious Realism."

As stated in the preface, religious realism means the view, "that a religious object, such as may be appropriately called God, exists independently of our consciousness thereof," of which, "it is possible for us to gain either (as some would maintain) adequately verified knowledge or (as others would be content to affirm) a practically valuable and theoretically permissible faith." The articles cover a wide variety of topics. Some of the most interesting are, "The Implications of Human Consciousness," by J. B. Pratt of Williams College; "A Phenomenological Approach to Religious Realism," by J. S. Bixler of Smith College, in which some characteristic ideas of Husserl, Scheler, Plato, and the Gestalt School are used in support of the thesis that the world, "as revealed in phenomenological intuition is a series of data which, in the religious field, are *a priori* true and, for the emotions, are *a priori* valid." "Religious Realism and the Empirical Facts of Religion," by

A. G. Widgery of Duke University, presents an interesting, partly historical, sketch of religious developments in widely separated religious groups, in each of which, he considers, is to be found some measure of objective truth as to the nature of the divine. "Plato as a Religious Realist," by R. L. Calhoun of Yale, is a well documented, but not very convincing, attempt to show that Plato's philosophy comprised many things which it possibly did not contain.

While there is a good deal in *Religious Realism* that may have bearing and effect on psychology, especially if the realistic movement continues to develop, and while the problems and discussions which the book contains are of considerable general interest and value, there is but a limited amount of material in the book which is of direct and immediate interest to the professional psychologist.

Hobart College FOSTER P. BOSWELL

Psychoanalyse und Pädagogik. By ERNST SCHNEIDER, Pädagogisches Magazin, Heft 1303. Langensalza, Beyer & Sons, 1930. Pp. 72.

This monograph is divided into three parts which are preceded by a brief historical introduction. In the first part is presented a series of "difficult pedagogical situations" together with psychoanalytical comments on each. These cases include such pedagogical problems with reference to children and adolescents as petty stealing, specific speech difficulties, difficulties with the multiplication table, etc. The second section contains an exposition of the theory of psychoanalysis, and the third part presents a brief statement of the value of psychoanalysis for pedagogy. There is appended a brief bibliography (forty titles) of psychoanalytic works in German of interest to the teacher.

The place of psychoanalysis in the theory and practice of pedagogy is indicated by the author's assumptions regarding the functions of pedagogical technique. Such technique, he defines, as the method of leading the individual toward a systematization of personal and super-personal interrelationships. The educated man must have achieved order within his personality and have integrated himself with society. Re-education becomes necessary when any degree of disorganization appears. This disorganization is indicated by the "symptoms" or disturbances which occur as the individual passes through the various stages of psychical development. These "symptoms" represent the attempt of the unconscious to substitute an unsatisfactory type of organization when the normal course of development has been blocked.

The value of the psychoanalytic technique for the teacher is found in the insight which it affords into a definite group of disturbances which hinder and endanger education as defined above. The symptoms of a diseased personality may be remedied by psychological and pedagogical methods directed towards the study of the unconscious. In addition the insight furnished by psychoanalysis will modify the attitudes of the teacher and hence his methodology so that he may be able to identify symptoms and prevent conflicts. Finally, psychoanalysis offers itself as a method which will remove repressed conflicts in teacher and pupil. Analytical procedures may be undertaken by the teacher himself when minor difficulties only are involved. More complete analyses must be carried out by the professional, doctor or pedagogue, who has no other relationship to the patient than that of analyst.

Critical comment appears to be superfluous. The author presents little that is new. The exposition of psychoanalytic theory follows closely the Freudian pattern,

and the applications to pedagogy are of the conventional type. The "difficult pedagogical situations" closely resemble the case studies with which the clinical psychologist is familiar under the term "behavior problems." For those persons to whom the basic assumptions of psychoanalytic theory are valid, the applications and point of view in the present monograph will be acceptable. For those to whom psychoanalytic theory is an interesting but experimentally unsupported group of concepts, the present monograph will offer evidence which is no more convincing than that presented in a score of publications during the past twenty years.

Northwestern University FRANKLIN FEARING

Speech Pathology. By LEE EDWARD TRAVIS. New York, D. Appleton & Co., 1931. Pp. xxxiv, 331.

This book presents a summary of much of the work which has been done by the group of workers who are interested in the influence of cerebral physiology upon the speech mechanism. It incorporates, with his own research findings, pertinent clinical and neurological observations, which are of much academic interest as well as of considerable clinical significance. The author holds that the neural mechanism of speech depends on a partial balance between the two sides of the cerebral cortex, in which one side must be dominant in order to have a proper coördination. When the individual is unable to allow his natural tendencies to express themselves in such forms as normal right or left handedness, there is interference with speech. The long exposé of the physiology of stuttering makes the book worthwhile in itself, presenting graphic evidence of defective synchronization of the thoracic and abdominal movements, tremors and tone-rigidity in the voice, abnormally long duration of tones, periodical fluctuation of breath-sounds and bizarre voice-wave forms to demonstrate cerebral influences. The author indicates briefly his findings regarding the abruptness of the emission of tones and repetition of sounds, words, phrases, as well as speech blocking. There is presented a summary of the influence of stuttering upon the patient with reference to secondary changes in his personality and social adjustment, and Travis' own opinion about the cause of stuttering. In the chapter on articulation and formation, as well as that on aphasia and kindred disorders, current thought on these subjects is briefly summarized. An appendix of 50 pages, consists of lists of stimulus words and sentences for speech training. Altogether, the book is an excellent and much needed contribution to the study of the mechanisms of speech. Considering the highly specialized nature of Professor Travis' research, the rather serious criticism of omitting any considerable discussion of the psychogenic factors in speech disorders can well be minimized.

Institute for Juvenile Research, Chicago LOWELL S. SELLING

An Experimental Study of Apparent Movement. By SUGI MIBAI. Psychol. Monog., Vol. 42, No. 190. Princeton, Psychological Review Co., 1931. Pp. 91.

This monograph contains the report of a series of experiments on apparent movement, as attacked by a new technique. The first part of the work is devoted to an historical summary of the findings of previous investigators, and the experiments are described and discussed in the later and major part of the study.

Mibai selected an Eastman kodascope as a suitable apparatus with which to demonstrate apparent movement. According to Eastman, this instrument has a speed

of 16 sections of film per sec., but Mibai found that the instrument used in his work had a speed of about 19 sections of film per sec. The length of pause between the two still presentations, which when made in proper temporal succession provided the conditions for apparent movement, was varied by changing the number of blank sections of film which were interpolated between the initial and the terminal picture. The two pictures were presented in different parallel and angular patterns in order to determine the influence of arrangement upon apparent movement. As compared with pieces of apparatus used in previous investigations on apparent movement, this apparatus offered the advantage of allowing repeated exposures of the pair of still pictures to be made.

Three problems were the particular objects of study. First, back and forth movement or movement back and forth from first and second positions. Secondly, rotary motion, especially the illusion of seeing a moving wheel spinning in the direction opposite to that in which it is going. And thirdly, comparison of apparent speeds.

True continuous movement was apparent when the two members were separated by 70 to 130σ. With a separation of 310σ, there was always mere succession of the pictures without apparent movement, and with a pause of 250σ succession was usually reported. Apparent movement was more clearly seen when the two successive members were presented in an angular than in a parallel arrangement. Conical movement, involving perception of the third dimension, was the rule when the angle made by the two members was greater than 90°. With novel patterns, movement tended to lack definiteness of direction even within the optimal conditions for apparent movement. Self-rotation, or rotation of a bar on its own axis, was reported when the temporal separation was short and when the distance between the exposures was small. The gray patch or flash, suggested by Dimmick as the correlate of *Phi,* was found to be very uncertain; it was not observed by all subjects, and was not consistently observed by the same subjects.

With regard to the illusion of seeing a wheel spinning backward when the movement is actually forward, it was determined that there were indifferent areas in the middle of the spaces between the spokes. When the spokes fell on these in succeeding exposures, the movement of the wheel appeared to fluctuate; when they fell on the spaces between the original positions and these areas, the rotation appeared to be clockwise; but when they fell beyond these areas, counter-clockwise movement was perceived. By increasing the number of spokes, the indifferent areas became smaller, and the correct direction of rotation was more favorably seen.

The significant findings in regard to comparison of apparent speeds were these: (1) fast motions were more accurately judged than slow motions, (2) speed tended to be judged in terms of time rather than in terms of distance, and (3) the speed of the second member tended to be accelerated.

Wellesley College MICHAEL J. ZIGLER

L'âme des Bêtes: quelques pages d'histoire. By L. VERLAINE. Paris, F. Alcan, 1931. Pp. 202.

This is a very readable history of animal psychology from the time of the ancient Greeks up to the end of the 19th century. The general theme of the book is based on the ancient contention that the major difference between animals and man is that the former are governed by instincts, while the latter are controlled by in-

telligence; or, in other words, that man has a soul or mind, a faculty which animals lack. During many centuries this idea seems to have been the basis for most studies in animal psychology, the works of those days consisting mainly of accounts of the marvels of animals instincts, and frequent controversies on the problem of intelligence. The author shows how the different movements of thought, philosophical and at times even religious, have contributed to this early idea that animals differ materially from man, and how they have altered this concept until gradually, through the influence of scientific ideas in general and the development of scientific methods in physiology, psychological interest in animals changed to other problems. Some of these new problems were: Are they conscious? Do they think? In what physiological and psychological attributes do they differ from man and from each other as one goes down the scale of animal life? Along with this change of interest there appeared also a tendency to apply scientific and experimental methods to the study of these various problems.

This book is apparently an account of the development of comparative psychology from philosophical speculation. Verlaine's method, however, may be compared with that of an orator who carries his audience to the climax of his story only to inform it that this will be the end. He stops where the story really begins to get interesting. Little or nothing is said about the many experimental studies on animals which have been carried out by European and American scholars since animal psychology has become a branch of scientific psychology. The development of animal psychology in the United States is not even mentioned; nor are the many studies which deal with the learning ability of animals, the complexity and limitation of the learning capacity, and also those which correlate learning ability with structure in order to discover how the development of the sensory, motor, and nervous equipment influences the adaptive or learning capacity. As far as it goes, the little book offers interesting reading for the philosophical-minded reader. The material is interestingly arranged and presented for the avowed purpose of "restoring the philosophy of nature."

Chicago, Illinois C. BURRI

Anger in Young Children. By FLORENCE L. GOODENOUGH. Minneapolis, University of Minnesota Press, 1931. Pp. xiii, 278.

This recent study of the child's emotions presents much interesting information relative to (1) the nature of angry behavior, (2) the frequency and duration of outbursts, (3) the conditions making for irascibility, (4) the immediate causes of anger, (5) the methods of control, and (6) parent-child relationships. The conclusions, carefully drawn and with an eye to their quite palpable limitations, are based upon reports involving some forty-five children ranging in age from seven months to seven years ten months. The records of behavior were made by parents on especially devised forms and cover periods of observation ranging from six to 133 days. These parents, because of their superior general academic training, were judged to be "at least equal in education and ability to the average of the technical assistants employed in most research laboratories" (p. 16). The results were pooled and analyzed in an attempt to bring out sex and age differences.

The most obvious criticism of the study is that the reliability of the basic data used was not determined. Some attention is devoted to this problem, but the fact

that records were complete was usually used as a basis for inferring that they were accurate. The report would have been considerably enhanced had two individuals observed the same child independently for at least a day and then compared records for consistency of observation. Illustrations of the lack of reliability of the evidence are apparent in a number of instances. For example, Table 3 indicates that some mothers of babies seven months of age observed them constantly for almost exactly the same number of waking hours per day as did those whose children were more than seven years old. Furthermore, there was so little correspondence between the mothers' statements (from memory) as to how they handled anger outbursts, and their actual technics reported at the time (p. 211) that the reviewer wonders if even these latter assertions were not quite generally rationalized. While the author thinks this improbable (p. 212), it is by no means an unjustified inference in the absence of any objective evidence to the contrary. A second limitation from which the study suffers, and which is candidly admitted by the author, is the paucity of subjects considered. This is quite obvious in cases where an attempt is made to locate sex differences in behavior at different ages from the study of groups of not more than eight subjects.

Anger in Young Children is called an exploratory study—as such it has value. No new technics of observation were developed, but an attempt was made to standardize reports made by parents, with the apparent realization that we must turn to this source for much of our information about child life.

It is to be regretted that the study was not planned to include both a determination of the reliability of the data and enough subjects to permit of some substantial generalizations.

University of Nebraska STEPHEN M. COREY

Statistical Analysis of American Divorce. By ALFRED CAHEN. New York, Columbia University Press, 1932. Pp. 149.

This is an interesting analysis of the divorce records of the United States Census from the Civil War to the present time. In this period the approximate increase of population was 300%, of marriages 400%, and of divorces 2000%. The divorce rate for 1000 married population has increased about 500% with a comparatively uniform rate of increase over six decades. At present the probability of divorce is approximately 18% of all marriages, and in 1929 there were 201,468 divorces granted in the United States.

The author adopts the standpoint, so prevalent in social and economic science, that statistical data possess distinctive factual importance and quite rightly therefore confines himself to a statistical analysis. The almost inevitable result is that he can formulate few positive conclusions as to the basic explanations of, or possible controls over, the prevailing condition. His summing up is that; "the rapidity of increase in the American divorce rate is the product of changing social conditions. The equilibrium of the family institution has been shaken by the radical transformations in its economic and social environment." This is not very helpful.

The value of the study lies principally in the discussion of commonly accepted explanations for the prevalence of divorce. In general these are shown to be inadequate. Some of his findings in this regard are that legal restrictions have little effect upon divorce rate. Establishing residence in another state for purposes of divorce ac-

counts for only 3% of total divorces. Only about one-third of the divorced people re-wed, which is only very slightly higher than for widowed persons. The urban divorce rate is about double the rural. Only 8% of marriages with children end in divorce as against 71% of childless marriages, and childless marriages account for 63% of all divorces. Economic production, urban growth, decreasing ratio of children and women workers "operate in the nature of concomitant or mutually interrelated forces as chief environmental causes in American family breakdown."

The study illustrates the limitations imposed upon an investigator who is dependent upon census data. These data are interesting but not very useful in bringing to light the basic influences that contribute to the prevalence of divorce in modern life. The necessary corrections and manipulations of the data are extensive but on the whole statistically justified. There is an extensive bibliography.

University of Toronto S. N. F. CHANT

Those in the Dark Silence: The Deaf-Blind in North America. By CORINNE ROCHELEAU and REBECCA MACK. Washington, Volta Bureau, 1930. Pp. 169.

In their study of "the most terribly handicapped of all human beings with intact minds, and the most neglected as a class," the authors have made the widest survey of this field that has so far been undertaken. They have recorded 665 living cases, with varying degrees of defect, from those totally deaf and totally blind, to those with minor degrees of either or both deficiencies. A practical classification in terms of causes is given: (1) the rare cases of congenital deaf-blindness; (2) the most numerous group—those who lost sight and hearing as a result of illness in infancy or early childhood; (3) the deaf who lose their vision and the blind who lose their hearing; (4) persons with normal senses who in adult life suddenly lose sight and hearing by illness or accident.

The first third of the book is devoted to an excellent picture of the status of the deaf-blind. Possibilities for pre-school treatment in the home are described, with suggestions for more adequate school training. In Europe there are already special institutions for the deaf-blind, but in America there is as yet no such provision for them. Until there is such special provision, the authors recommend education in schools for the deaf, in order to learn means of communication; 11 methods for conversation and 13 methods for reading and writing are described. Training can then be continued in schools for the blind.

The authors point out certain objections to "psychological diagnoses," until special intelligence tests for the deaf-blind have been developed. The results of tests not adapted to their special needs may destroy confidence in their potential abilities. There have been cases in which children have been consigned to institutions for the feeble-minded as hopeless custodial cases, although their later history proved them to be educable.

The problem of work for the deaf-blind is well presented, and suggestions made for the coöperation of state, church, and welfare societies. For example, various religious bodies might each assign one person to the special ministry of this neglected group. "One priest, one minister, and one rabbi could take care of them all," by correspondence with the individuals and with their local agencies when personal visits are impossible. Until a national institute devoted to their general welfare is established, the authors offer this practicable plan for bringing to the

deaf-blind the pleasures of friendship, the consolations of religion, and contact with the outside world.

The latter and larger part of the book consists of a biographical section where notes on the cases are given, ranging from the mere listing of names or initials to accounts covering several pages. The remarkable attainments possible after proper home and school training make a forceful plea for the extension of adequate opportunities to all deaf-blind children.

The book is a valuable and timely contribution to the growing literature upon the handicapped.

Mount Holyoke College SAMUEL P. HAYES

Nightmare, Witches, and Devils. By ERNEST JONES. New York, W. W. Norton & Co., 1931. Pp. 374.

The main body of the book is divided into four parts. The major topics are respectively the Pathology of the Nightmare, Connections between the Nightmare and Mediaeval Superstitions, the Mare and Mara, and the Conclusion.

The book was written, mainly in 1909 and 1910, from the point of view of Freud's school of psychoanalysis, and the treatment follows the lines suggested by Freud in his early assumptions. For example, the statement is made that Nightmare is always an expression of intense mental conflict centering about some form of "repressed" sexual desire and that the repression concerns incest trends of sexual life. Similarly the claim is made that the Devil represents in the main an exteriorization of the repressed wishes which are ultimately derived from the infantile Oedipus situation. The Witch belief is also analyzed as an exteriorization in the main of the repressed sexual conflicts of women, especially those relating to the feminine counterpart of the infantile Oedipus situation. In short, the conclusion is that all mediaeval superstitions are of incestuous origin.

The point of view upon which the statements are based would neither be accepted by most American psychologists today nor by some of the "depth" psychologists. About 500 authors, not experimental psychologists, are cited to suit the purpose of the discussion and many of the fundamental assumptions are obviously hypotheses. But Jones passes serenly on without apparently realizing that his tenets are prejudiced or speculative in the least.

It will be difficult to determine the ultimate value of the publication before psychoanalysts develop their views further on some factual basis and before experimental psychologists do more in the way of examining the mechanisms involved.

Eastern Kentucky State Teachers College NOEL B. CUFF

Personality and Will. By FRANCIS AVELING. New York, D. Appleton & Co., 1931. Pp. x, 246.

To have personality or to be a person is, according to Aveling, to have a soul. This soul or profound self is given in immediate experience, especially during acts of will, and is characterized "as a substance, or entity subsisting in itself: as an activity immanently energizing, or originating and terminating its action within itself: as a being altogether incommunicable to any other being." The experience of this self is non-senorial or super-phenomenal, "a unique experience in which a noumenon may be said to be identical with its non-sensorial phenomenon," but withal an ex-

perience "which is as empirical as any experience can be." This soul is inseparable from the body and in fact necessary to the very existence of the body. The body is "an immense aggregate multitude of energies which are qualitatively, quantitatively, locally and temporally indetermined," while the soul is "a substantial activity, likewise energetic." The energy of the soul systematizes and integrates the indetermined energies of the body into a unitary living organism. "The soul is not the lute, nor the harmony of the lute; though lute and harmony are both involved in personality. It is that which constitutes the lute an instrument of harmony. Neither the body nor the soul is the person. The person or self is the organic unitary whole, the resultant of a multitude of energies, systematized by a constitutive principle which is conscious energy itself."

Such, in brief, is the thesis of Aveling's book. The problems dealt with—the nature of the soul, the manner in which it is revealed, and its relation to the body—are problems largely foreign to American psychology. The proposed solution to the mind-body problem is, as far as the reviewer knows, a new and original one, but one which is not convincing. In all biological structures there is a multiude of energies which are synthesized and integrated, but surely every such structure does not possess a soul or a conscious energy which integrates this multitude of part energies. The author's method is old; it names a faculty or soul, but fails to indicate how this entity does its work. Science is interested in the 'how.'

Stephens College CARL N. REXROAD

Clinical Psychology. Studies in Honor of Lightner Witmer. Edited by ROBERT A. BROTEMARKLE. Philadelphia, University of Pennsylvania Press, 1931. Pp. xxi, 409.

Of the two *motifs* recurring in and dominating this commemorative volume for Professor Witmer, one is, not unnaturally, Lightner Witmer. The other is that emphasis and stress on individual psychology as against statistics, on diagnosis contemplating the total personality, which is so much the clinical psychologist's way of thinking, but still relatively foreign to the interests of many followers of psychological science. The area of the clinical field covered by the 30 articles constituting the volume may be estimated from the list of topics considered: the preschool child, the superior child, mental deficiency, speech defects, psychobiochemistry, industry, diagnostic education, criminology, conduct problems, religious experience. Among the authors are Fernberger, Maxfield, Miles S. Murphy, Brotemarkle, Buzby, Viteles, Twitmyer, and Gault, while Professor Witmer himself is represented by three reprints, dated respectively, 1907, 1915, and 1925. The volume also brings into historical perspective a special sector of American clinical work, tracing its relationship to experimental psychology, through the Witmer paternity, to Wundt. Hardly so much stressed as present circumstances would warrant, is the effort to bring into similar perspective the position of clinical psychology in relation to the associated activities of psychiatry and social work. The coöperation with (and often lack of it from) school systems is more adequately treated. One should add that the trained clinical psychologist will find in the volume little that is new. Certainly little that to him ought to be new. The reader will find, too, much overlapping of thought and principles expressed, and of methods and objectives described. This is, however, almost inherent in a collection from so close-knit a group all writing around one central topic.

Not so inherent, in a group of scientific writers, should be the recurrence of that other mentioned *motif*, adulation not far from idolatry; father-introjection would be a psychoanalytic name. Professor Witmer offers in the following litany more concise expression of the theme his disciples are chorusing; (388 f): "Whenever I think of the dead weight of obscure words defining mystical metaphors, like the 'group mind,' . . . Whenever I consider the often equivocal sociological statistics which pass current as 'educational psychology,' or which impose I.Q.'s with a soothsayer's finality upon the ignorant and undiscerning, and . . . whenever I contemplate the mountainous labors which produce the usually ridiculous mouse called 'a coefficient of correlation,' . . . I pray for all the guidance necessary to avoid futility and dullness—enough analytic discrimination to make clear a few ideas, and the artists' choice of the best words in which to clothe them." Among these artists' words, be it noted, are "psychonomic personeering."

Boston Psychopathic Hospital SAMUEL J. BECK

A Mind that was Different. By DOW THOMPSON. Introduction by M. V. O'Shea. Oklahoma City, Harlow Publishing Co., 1931. Pp. xiii, 117.

Even though O'Shea says in the Introduction that this book "can be commended to the psychologist who is searching for reliable data bearing upon the traits of the introvert and also of the extrovert," and characterizes the author as an "enthusiastic *psychologist*" (italics mine), the book is really not scientific or psychological at all. It contains observations, interpretations, and opinions, and a number of clear rationalization processes to account for personality differences between the author and a brother. The former is characterized as introvert and the latter (who "made friends easily and naturally,") as extrovert. The account is traced in a general way through years of development and the author characterizes the different personalities of the brothers in terms of reactions to various situations confronted. The author came gradually to a realization, from readings and observations, that instead of being peculiar or 'different,' he was only like a certain class of individuals, whom—following C. G. Jung—he calls introverts. For him this is a permanent innate trait. There is little recognition that one may in different circumstances, despite any general disposition, show now introvert and now extrovert reactions. For the author, the distinction between these two types is so easily made that he proceeds—once he has characterized the types—to select a considerable number of historical examples. Some introverts selected are: Coolidge, Hoover, Theodore Roosevelt, Henry Ford, John D. Rockefeller, Sr., U. S. Grant, Ferdinand Foch, Edison, Charlie Chaplin, Benito Mussolini, Gustave Stresemann, etc. Indeed, it seems to him that most great leaders should be included. They are individualists, show a tenseness or need of relaxation, prefer to live by themselves, are not able to subordinate their opinions to those of others, manifest indifference to the views of others, etc. But often with great leaders (introverts) there are closely associated persons who get on well with them because they are their opposites, or extroverts. Thus with Pierre Curie was Madame Curie; with William James was his brother Henry, etc. Here the author made another 'discovery:' "the universality of nature's law that introverts marry extroverts" (p. 66). He even finds many analogies to this law in the inanimate world. Going into the matter of marriage between the two types, he found introvert-extrovert mates in his own immediate family, in his brother's family (his

brother, although he had "kept company in his youth with extrovert girls, selected an introvert for his wife"), in his sister's family, in his father's family, and in many historical families. No; Nature's universal law is not *always* followed; a few persons make errors—due to other complicating factors—and consequently do not get along well with their mates!

Another discovery: Nature has provided that children in the same family alternate in type so that if the first is an extrovert the second must be introvert, etc. Altogether, though it is admitted that there are some weak introverts, the author seems to find comfort finally in the discovery of the type to which he belongs. A number of typographical errors occur in the book.

Unfortunately, this sort of enthusiastic 'psychology' is of no value even to the lay reader; and it may do him an injustice—particularly if he believes what he reads.

J. P.

Speech and Voice. By G. OSCAR RUSSELL. New York, The Macmillan Co., 1931, Pp. xvii, 250.

The bulk of this book is concerned with an interpretation of over 100 X-ray pictures of the vocal and articulatory mechanism. A phonographic record, synchronized so as to record the voice produced during exposure, was made simultaneously along with each X-ray picture. The outstanding single conclusion is that while the tone remains the same the cavities may be altered and that neither a single individual from time to time nor different individuals take the same position for a given cavity in order to produce a given tone. However the author does concede that some given characteristic seems typical of the position of the cavities when a given vowel is sung or spoken.

More than any other writer, Russell emphasizes the influence of surfaces (hard and soft), the false vocal cords, the epiglottis, the ventricles of Margagni and the glottal factor in tone formation. Narrowing of soft surfaces (lips, tongue, velum, faucal pillars, soft palate) mellows a tone and exposure of hard surfaces to tone waves makes the tone metallic. The former is accomplished by "deadening," and the latter by accentuating, the high partials. The false vocal cords and the ventricles of Morgagni operate in the creation of low-pitched guttural and hoarse tones. The epiglottis functions as a soft-surface filter to lower pitch and mellow tonal quality. Bony cavities (sphenoidal, frontal and maxillary sinuses) are of little or no importance.

There is a chapter on X-ray pictures as aids in training the deaf and several chapters on the physiology and anatomy of the peripheral speech mechanism.

Menville and Ane (*Proc. Soc. Exper. Biol. & Med.*, 29, 1932, 825-826) seem to have an improvement over Russell's mechanical means of outlining the forms and sizes of the speech and singing cavities. They used a preparation of glucose and bismuth. Parmenter and Trevino (*Quar. J. Speech,* 18, 1932, 351-369) have questioned all of Russell's X-ray findings. They suspect that Russell did not keep the subject in the same position for the different radiographs. Consequently comparisons from one picture to another for the same subject or for different subjects cannot be made. Further they found in contradistinction to Russell that each individual takes a fairly definite tongue position for each vowel. Regardless of how this controversy is settled Russell deserves much credit for his extensive investigations.

University of Iowa LEE EDWARD TRAVIS

Order of Birth, Parent-Age, and Intelligence. By L. L. THURSTONE and RICHARD L. JENKINS. Chicago, The University of Chicago Press, 1931, Pp. xiii, 135.

The first part of this book reports an investigation of the relation between the order of birth and the intelligence of children, and such other related problems as fecundity and intelligence, effect of sex on siblings, ages of parents and intelligence of children, and time interval between successive births and intelligence of children. These investigations were carried out on data collected at the Institute of Juvenile Research, Chicago. A sample of about 1500 cases in chronological order was drawn from some ten thousand case-records on file at the Institute. The mental ages and intelligence quotients used in the investigation were obtained by individual psychological examinations, generally with the Stanford-Binet examination.

The most important study is the first one named. In order to rule out the effect of social and economic status, it was limited to siblings. A comparison of the average I.Q. of first- and second-born in the same family, first- and third-born, etc., including eight birth-orders, shows a significantly higher average I.Q. and a greater variability of test intelligence for the later-born children. An examination of the internal consistency of the differences between the weighted means of various birth-orders involved in each comparison indicates a definite increase in intelligence with birth-order.

In the related problems studied, some of the more important conclusions are: (1) There is no definite relation between individual ages of parents at the time of birth of a child and the intelligence of that child, but there seems to be a tendency for children of parents widely separated in age to be mentally inferior to those of parents more nearly the same age. (2) Twins do not appear to be inferior in intelligence to their non-twin siblings, nor is there any difference in this regard between same-sexed and opposite-sexed twins. (3) There is seemingly a better prognosis for the behavior adjustment of a child that has an opposite-sexed next older or next younger sibling than of a child with a corresponding same-sexed sibling. The authors do not attempt to explain the results of these investigations, but certainly environmental factors need more careful examination.

Part II is devoted to a review of other studies bearing on the problems reported in Part I. There is a bibliography of 167 titles.

University of Kentucky E. J. ASHER

Delayed Reactions in Rats. By C. H. HONZIK. University of California Publications in Psychology, Vol. 4, No. 19. Berkeley, The University Press, 1931. Pp. 307-318, 1 fig.

Honzik tried out, in experiments on 21 rats, his idea that if rats, before the delay period, were allowed to run up to the stimulus this might not only increase the certainty that they actually received the stimulus but it would also serve to reënforce the stimulus and thereby possibly extend the delay period. A box with three curtained doors (two black and one white) was used. The white curtain, which was to be the cue to food via an elevated runway behind this curtain, was shifted in a random order according to a chance schedule. The animals were habituated to the situation before the experiment began. Motivation in the preliminary training to go to the white-curtained door was food for right and a mild electric shock for wrong choices. Then, after some overlapping of training and delay tests,

the delay period was determined by letting the rats run up to the white-curtained door. Here their entrance to the food was prevented by a block attached to the lower end of the curtain. An opaque detention screen just before the curtain was then lowered, delaying the rat any predetermined period of time but leaving it free to run about in the box ahead of the screen. Meanwhile the white curtain and block were raised, bringing into position another black curtain (attached to the block) so that the rat, when released, had to choose among three similar black curtains of which only the one that replaced the white would yield admittance via the runway to the food. Various precautions were taken to prevent visual, auditory, or olfactory cues and to force the animals after the delay to use whatever other 'guiding cue' they might have. Delays of at least 45 sec. (possibly 3 min.) were found to be possible for most of the rats, with correct responses "somewhat higher than would be expected by chance." Two repeated studies by the author, involving slight modifications and new sets of animals, supported the results of the main experiment.

Differences between the behavior of successful and of unsuccessful rats are mentioned. "The better rats would often pause before a curtain or before two curtains in succession before finally choosing the correct one, the poor rats would undiscriminatingly poke their noses under the nearest curtain and very often return to a curtain that they had already tried" (p. 317). It is unfortunate that such observations are merely general, verbal statements more or less casually made, and that the reader does not have detailed data on other important matters so that he can analyze the results himself in an objective manner. Unless other experiments can present such details, obtained by objective and accurate technique, this interesting experiment will lose much of its scientific value.

J. P.

Policy and Ethics in Business. By CARL F. TAEUSCH. New York, McGraw-Hill Book Co., 1931. Pp. xi, 624.

This book is a plea for autonomy in business. The supporting argument is straightforward: (1) business, if controlled internally by proper ethical standards, should be exempt from interference from the outside; (2) to realize this ideal business must defend itself against forces within itself and also against other forces on the outside which, if not put under proper control, would cause it to operate at less than a reasonable profit—but the profit must be reasonable because it is bad taste to retain more than a reasonable profit; (3) in the achievement of the autonomy to which business is justly entitled, the constructive task will be the establishment of a standard of profit which will be defensible by the principles of ethics; (4) such a standard, safe for business and harmless to society, must be constructed through a complete knowledge not only of the intrinsic nature of business itself but also of all pertinent extrinsic factors such as the racial, geographical, religious, and social features of the people themselves. The book is not purely theoretical. At all points the argument is supported by a comprehensive structure of facts.

What the psychologist may glean from the argument and the facts will depend upon his own conception of what constitutes the psychological. There is no section of the book devoted exclusively to the psychological principles in business; but for those who find psychology in all human relations, abundant material is offered.

Wittenberg College H. G. BISHOP

The Experimental Study of Reading. By M. D. VERNON. Cambridge, Cambridge University Press, 1931. Pp. xv, 190.

The author attempts "to give a concise account of any experimental work . . . which throws some light upon the psychology of reading." This statement, however, indicates a more comprehensive treatise than is produced. In general, eye movements, perception, and legibility in reading are treated, with more space devoted to the first which is the author's own field of study. Methodology is emphasized. It may be characterized as a good book, but one which contains several inadequacies. Many of the analyses reveal alternations of fine critical insight and credulous acceptance of unwarranted statements or hypotheses. A lack of thoroughness is manifested by omission of both citation and discussion of many investigations whose consideration would have, in some instances, modified the author's conclusions. This is particularly true of the section on reading disabilities. The parts on visual perceptual processes in adults and in children, while adequately done, are somewhat superfluous and add little or nothing to the discussions which follow on visual perception in reading.

The critical reader will find much to commend in the book, especially in the discussions of various aspects of eye movements. One should mention in particular the important analyses of subjective accompaniments to variations in eye movements. The influence on ocular behavior of interest, relevant and irrelevant associations and imagery, and affective reactions, are cited from Vernon's own work. Among other things she rightly points out that a moderate kind of irregularity in reading movements need not be "inconsistent with the best kind of critical appreciation; and it may indicate the existence of illustrative thought and rational reflection."

University of Minnesota MILES A. TINKER

Englisch-Deutsch für Psychologen. By JAMES A. HAMILTON. Frankfurt a. M., Germany, Gaul & Bantelmann, 1931. Pp. 103.

This dictionary of English psychological terms, translated into the equivalent German terms, ought to prove particularly useful to foreign scholars who have a better command of German than of English. Some two thousand words are given and the book is printed in excellent Roman typography with the English words in black face capitals. The gender of the German nouns is indicated. The definitions though concise, restricted as they are for the most part to one or two equivalents, are, nevertheless, adequate. The reviewer, who is also the author of a German-English dictionary of psychological terms, realizes altogether too well the difficulty of having the first edition of this dictionary letter-perfect. But errors will creep in, as, for example, the misspelling of Ebbinghaus's name on page 24. There are quite a few terms in the list that are simple transliterations, like "shock" = "Schock." On page 76 there are at least six such simple transliterations. The compiler shows a considerable amount of psychological insight as well as a command of both languages. The booklet should be in the hands, not only of those psychologists who would become more familiar with the English language, but also those American and English students of psychology who wish to check back their knowledge of German terms. Few people who have not undertaken tasks of this sort can fully appreciate the tremendous amount of accurate and painstaking work which lies buried in the compilation of these hundred pages.

University of Iowa CHRISTIAN A. RUCKMICK

The Person of Evolution; The Outer Consciousness; The Outer Knowledge; The Directive Power. By W. D. LIGHTHOLL. Toronto, The Macmillan Co., 1930. Pp. 216.

This is a book of "studies" of consciousness in evolution privately published as *The Outer Consciousness* in 1926, but now including "several articles published in *The Philosophical Review* and other places." It is not a scientific book, the propositions being only the author's "own judgments" as stimulated by "the latest discussions of the best authorities on the several sciences" (p. 13).

The interests of the author are fundamentally theological, and the book is one of fantastic, untestable hypotheses. 'Instinct' is an independent drive in the individual from without. It operates toward 'paths of joy.' "throwing out as its organs, conscious individuals to pursue them" (*i.e.* these paths). This drive turns out to be "a Power conscious of what it 'drives' towards," God, "the ultimate Person of Evolution," etc. This conclusion is forced on the author by *"the ingenuity of the innumerable contrivances* which favor joy and avoid pain, through all the history of terrestrial life." Teleology, the transmission of acquired characters, etc., are assumed in the face of established scientific facts to the contrary, and consciousness is taken as a *vera causa.* There is an extensive literature today of this sort, distorting scientific facts to meet ulterior ends, or to follow personal feelings. Authors are not always so frank to tell the reader that they are giving merely their own opinions.

J. P.

Prediction Methods and Parole. By GEORGE B. VOLD. Hanover, The Sociological Press, 1931. Pp. xvi, 138.

The author of this study has adapted the modern methods of rating and scaling to the problem of predicting the successful adjustment of paroled prisoners. As stated in the Preface this study "attempts to show how the information already in the parole records may be utilized for a more systematic and intelligent selection of the men to be placed on parole." The method was taken largely from the pioneer studies of Burgess and Glueck. The results, in agreement with the previous studies, indicate that no one factor is of outstanding importance in predicting violation or non-violation of parole. However, the author does find that a cumulation of factors, either weighted or unweighted, is quite significant. A scale is presented which has a practical value in that data on records of a parole department are used. This should be of special interest to those concerned with the general supervision of the parole of prisoners. The author suggests, as the logical next step in the development of a more scientific parole procedure, the application of the Experience-Expectancy parole violations at the time of granting the parole.

North Carolina State College K. C. GARRISON

The Development of the Sexual Impulses. By R. E. MONEY-KYRLE. New York, Harcourt, Brace & Co., 1932. Pp. 219.

An excellent synthesis of sexual psychology from the viewpoint of psychoanalysis. In spite of its highly speculative nature and of the unverified assumptions, it should prove both interesting and valuable to those interested in the sexual factors in personality.

Trinity College R. B. W. HUTT